A RATIONAL INVESTIGATION OF THE PRINCIPLES OF NATURAL PHILOSOPHY,

PHYSICAL AND MORAL.

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"Wake all to reason;—let her reign alone."—Young.

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PREFACE.

By Philosophy, in the following Investigation, we are to understand the Knowledge of Causation, or the Relation of Cause and Effect;—not merely the knowledge of things and events, but the way in which things are formed, and events brought about. With regard to the nature of this knowledge, and the way of attaining to it, much difference of opinion has been entertained by different men, and at different periods of the world. We may observe in general, however, upon the course that has hitherto been pursued, that there has always been amongst philosophers, as amongst other classes of mankind, much vanity: They have shown a stronger disposition to assume an intellectual superiority over others, than to benefit them by their labours.

Previous to the times of Lord Bacon, science consisted of little else than mere hypotheses. Each philosopher, in his turn, contrived some new hypothesis, upon which, as a foundation, he employed all the powers of sophistry to raise some plausible theory, wherewith he amused himself and his followers, and acquired a certain kind of fame, but afforded little, if any, benefit to mankind. Upon the discovery of the art of printing, and the consequent general dissemination of knowledge amongst

all classes, scientific men found that they could no longer support their credit in the world by these fanciful and delusive theories, and that some other means must be resorted to. Lord Bacon then ventured to introduce a mode of philosophizing very different from any that had till then been thought of, founded upon experiment and actual observation. Since the introduction of this new era, scientific men have been busily employed in collecting and arranging facts, and propagating the knowledge of these facts; but they have almost altogether laid aside theory. They have thus collected, and disseminated amongst all classes of men, much valuable knowledge, certainly; but they have not equally promoted true philosophy. They give us a knowledge of the existence, but not of the nature of things. They give us a knowledge of events, but they do not tell us how these events are brought about. In short, they do not afford us a knowledge of the *system* of nature. The knowledge of these philosophers differs from that of the mere practical man only in being more extensive and general—extensive in breadth, but not in depth.

The imperfections of such a system, if a system it may be called, are many and great. When we attempt to apply, in practice, the information thus afforded us, but under some variation of circumstances, (as must almost always be the case more or less,) we are left quite at a loss how to vary the means to bring about any required result, having no certain rules to direct

us. If experiments are to be used without theory, an experiment must be made for every possible case that may occur in practice. But this would evidently be superseding philosophy altogether. If experiment be all that the practical man is to receive from the learned, every man must naturally feel more interested, and have more confidence in those experiments which he is every day deriving from his own profession, than in those of others, which must always be more distant from, and less applicable to, his particular purpose.

The proper business of the philosopher is to unravel the *order* of nature, and to discover those *laws* by which all its phenomena are regulated. The experiments made by him ought always to be with a view to the discovery of those laws. Unless we can trace these, the experiments themselves are of comparatively very little use. Unless we can discover the laws which regulate the phenomena, experiments, when made under a variation of circumstances, frequently afford such seemingly different results, that they tend rather to perplex and confound, than to form a guide for our future practice. It is the knowledge of the order, or the laws of nature alone, that is useful in a scientific point of view; and it is towards the attainment of this that all the experiments of the philosopher ought to be directed. It is by this that things, seemingly the most intricate and complicated, are rendered plain and simple; and the practical man receives instruction how to vary the means according to the object he has in view;

or he knows the result he has to expect, from the means which he must employ. The practical man very wisely puts less confidence in any theory, until it is confirmed by facts, than in the experience which he has in his profession; but for the philosopher to despise theory is the grossest absurdity; for in it lie the whole value and importance of his profession. Even hypothesis is not by him to be altogether laid aside. In fact, hypothesis forms a necessary step in the acquisition of scientific knowledge. Experiment and observation form the foundation of all. From these we form conjectures as to the causes of things; and these conjectures are at first neither more nor less than mere hypotheses. It may, indeed, be an evident fact, that a certain body is the agent that produces the change upon another body acted upon; but the *way* in which we suppose the change to be effected is at first commonly a mere conjecture, or hypothesis. We next proceed to examine more minutely the *modus operandi;* and if we are able to trace the course of the operation, and find that our first conjecture was right, then, from what was at first only an hypothesis, we are enabled to form a demonstrative theory. Hence, those philosophers who adhere to mere induction, and refuse to proceed to theory, are most inconsistent in condemning hypothesis; for their induction is mere hypothesis. Induction from experiment and observation is the first step in the acquisition of knowledge; but if we proceed no farther, our knowledge must be very imperfect. By induction we raise

ideas, and form conjectures; but without the test of demonstration we never can be sure that these conjectures are right. By induction we dig the ground and collect materials; but without demonstration the fabric of a system never can be reared. To rest satisfied with mere induction would be to stand forever upon the threshold of knowledge, and never venture in.

But although modern philosophers are constantly disclaiming all pretensions to theory, it is evident that they are continually aiming at it more or less. They would give us theories enough; but they are unwilling to encounter the labour that is necessary to bring them to perfection—that labour that is necessary to bring to perfection every art and every science. As the foundation of mechanical philosophy, they have given us laws of motion, which correspond very well indeed with the phenomena of motion, but not with the effects of moving power, as applied to the useful arts; which constitutes by far the most important part of the science. The system of mechanics, founded upon these laws, is found to be of little or no use in practice. Although it has long been a peculiar object with the learned to instruct and direct the practical mechanic, their speculations have generally been despised and neglected, as of no earthly use to the man of practice; and fitted only for the amusement of the idle and the curious. Philosophers have attributed this reception which their labours have met with from practical men to the ignorance and obstinacy of the latter. But the imputation

philosophers who scout this hypothesis as to the cause of gravitation, have adopted one exactly similar as to the cause of repulsion, or heat. This is supposed to be produced by a matter *sui generis*, termed caloric, which must operate in a way no less mysterious than the gravitating ether. Both hypotheses are alike; and both deserve the same fate.

It is not merely with regard to gravitation and repulsion that this Investigation confutes the doctrine of absolute power: It is one of the principal results of it, that there is no such thing as the exercise of absolute power discoverable in the whole economy of nature, either in the physical or the moral world.

The application of these forementioned hypothetical experiments by the author, to the planetary system, most evidently pointed to the common origin of all its parts. On comparing the theory thus suggested, both with the general phenomena of the planetary system, and the particular constitution of our own Earth, he found the agreement so striking and uniform, that extension of inquiry and comparison, only tended to confirm it. He was at the same time, certainly, not a little gratified to find that this theory embraced, and brought together into one system, the leading points of the theories of the most eminent authors who have treated on this subject, viz. Buffon, Hutton, De Luc, and Cuvier; however these theories may differ, in many respects, from one another. It is hoped that this will at last form a real foundation for a Theory of the Earth; and render that a subject fit for scientific inquiry, and

by no means beyond the reach of the powers of the human mind; which has hitherto been considered as little else than an exercise for the fancy.

The author could have wished much to have added something farther on the theory of heat; but this he candidly acknowledges he has been unable to effect. It is on this account that he has declined touching on the steam-engine, certainly the most valuable engine ever invented. Without some farther knowledge on the nature of heat, any improvement on this engine is perhaps rather to be looked for from the civil engineer than the mechanical philosopher.

The scholastic reader, who has been accustomed to consider Mechanical Philosophy as a mixed mathematical science, will no doubt look with surprise, or, perhaps, with ridicule, upon the attempt to investigate the subject with the employment of such a small portion of mathematical science as that made use of in the following work. But however necessary the most profound knowledge in that science may be for the completion of a *system* of Mechanical Philosophy, it is not so for understanding the *principles* on which such a system is founded;—although necessary for the particular solution of mechanical questions, as the result of the operation of certain forces acting in given circumstances, it is not necessary for understanding the way in which the cause is related to the effect; which is the object of the following Investigation, and which has been performed without any knowledge in geometry beyond the 47th Proposition of Euclid. This has, indeed,

frequently caused the author, in the solving of different questions that occurred, to have recourse to laboured approximations. But although this may not be deemed a very scientific mode of proceeding, it is hoped that it has not led to any very important errors—any that can at all affect the general reasoning.

The author is quite aware, that, from a certain quarter, opposition will be made to that part of the Investigation which treats of the Origin of the Planetary System, from its complete disagreement with the account given by Moses of the creation of the world. But if this objection is to be sustained, we may, for similar reasons, reject the whole Copernican system of astronomy.

Whilst the author was engaged in these physical inquiries, he happened at the same time to be taking a particular interest in the study of Moral Philosophy. He had not proceeded far in the latter when he found the most striking analogy in the laws that regulate the two great departments of Nature, *Matter* and *Mind;* and the strictest scrutiny satisfied him that throughout their whole extent the analogy is complete; and that even man, who holds the highest rank among created beings on earth, is directed in all his actions by the same law of relative necessity which regulates the motions of the material system. This has generally been considered a dangerous doctrine; and perhaps in the way in which it has hitherto been treated, not without reason, as leading to materialism, and doing away the responsibility of man. But this proceeds entirely from the erroneous manner in which the subject has been

treated. The analogy betwixt the laws that regulate matter and mind never can confound the one with the other. So far, indeed, is the following Inquiry from leading to materialism, that one principal result of it is, that all the properties of matter are to be resolved into the operation of mind; and that there is no such thing in nature as matter absolutely inanimate. The moralist has no reason to be afraid of any danger to the cause of morality from the doctrine of necessity, as it shall appear in this Inquiry. On the contrary, it points to the only sure and effectual means for improving and perfecting the morals of mankind. It shows that similar motives, operating upon similar minds, will produce similar effects; but it does not follow that the same motive will produce the same effect upon every mind; for the effect depends as much upon the state or nature of the mind operated upon as on the motive applied to it. And owing to this very circumstance, the certain operation of external influences, the constitution of the mind itself is much under the control of those who have the charge of the education of youth; and hence the mind can be prepared to resist the force of external motives and temptations. Man is not less provided with safeguards against the dangers which assail the mind than those to which the body is exposed. The application of fire will inevitably burn a man, and immersion in water will certainly drown him; but he is gifted with eyes that he may see his danger and avoid it. To guard him against mental dangers before they arrive, as well as for many other important purposes, man is endowed with a faculty of the mind exactly

corresponding to the bodily eye. This is ***Reason***, o the ***Eye of the Mind***—a faculty which man, by the despotism of superstition, has hitherto been prevented from using, or even from being aware of its true nature and use. Hence, with whatever invariable certainty motives, when they come into operation, will produce their natural and legitimate effects, yet the man who commits a wrong action can never plead non-responsibility, so long as he is endowed with reason to warn him of his error. He may frequently, indeed, throw back part of the responsibility on those who have had the charge of his youth, in so far as his errors may have proceeded from a wrong education: but, however much they may lament his misfortune in this respect, that will never prevent others from considering him, as he really is, a bad man, or a dangerous member of society, and justly deserving of punishment. It is only when a man's reason fails him, that he is considered to be not responsible for his actions. Prudence may then require him to be confined; but whatever may have been the nature of his actions, reason does not discern him to be a fit object for punishment.

The necessary connection betwixt cause and effect in morals, never can alter the opinions of mankind with regard to right and wrong;—never can prevent man from pursuing what is good, and avoiding what is evil; and so far is it from discouraging him in using means for the improvement of the morals of others, that it holds out the greatest encouragement for their use—the certainty of the effect, when the means are properly applied. In short, by making morality rest

upon the necessary influence of external motives, we place it upon its only true and natural foundation—we place it upon the principle of moral attraction, exactly corresponding to the simple and sublime principle of physical attraction, which upholds and regulates all the motions of the physical system of the universe.

The author is certainly justified in saying, that no inquiry was ever conducted with less prejudice than this. He could have no motive to bias him in bringing out any of the principal results: for he had not these at all in view as the objects of inquiry. He was not inquiring whether Absolute Power was the principle on which the Deity conducted the affairs of the world, when he found that all the phenomena of nature were to be traced to the principles of action and reaction. He was not inquiring into the Cause of Gravitation, nor the Origin of the Planetary System, when he was accidentally led to these from certain hypothetical trials on a system of unequal bodies moving in equilibrio round their common centre. Neither was he inquiring whether man was a Necessary or a Free-Agent, when, on examination of his moral constitution, he found him in all his actions to be actuated by the same law of relative necessity as those other parts of the system of nature, commonly termed inanimate. In fact, he had no determinate object at all in view but to investigate the System of Nature, by following the truth whithersoever it might lead, under a perfect confidence that the truth would ultimately lead to the general good of the world. How far he has been successful in the development of the laws of nature, he must leave to

others to determine. In the mean time, he rests the hopes of his success on the uniformity of the reasoning, and the unbroken thread of argument he has been enabled to keep up from beginning to end; and, above all, on the agreement of the theory with the phenomena of the world. Nothing but the most perfect confidence in the truth of the results of this Inquiry could ever have induced him to lay it before the world: but however confident he may be of the rectitude of its general principles, he has not the most distant expectation of giving to the world any thing like a perfect work. On the contrary, every time he has revised it he has discovered many errors; and he has no doubt that it still contains many more. The detection of these, however, will give him no uneasiness: he will rather consider himself indebted to those who will detect and correct them. As his sole object is the discovery of truth, he will deem it of very little consequence whether he shall effect this by his own direct means, or by stimulating and encouraging the exertions of others.

He now submits the result of his labours to his countrymen, in the hopes that it may be read, as it has been written, without prejudice, and with an attention proportioned to the importance of the subject, rather than to the manner in which it is here treated. As Natural Philosophy comprises the foundation of all useful knowledge—the knowledge of every thing in which man can have an interest or concern, the subject is certainly the most important and interesting that can occupy the attention of the human mind.

PART I.

INVESTIGATION OF THE PRINCIPLES OF MECHANICAL PHILOSOPHY.

"Has matter innate motion? then each atom,
Asserting its indisputable right
To dance, would form an universe of dust:
Has matter none? then whence these glorious forms,
And boundless flight from shapeless and reposed?"

Night Thoughts.

CHAPTER I.

THE LAWS OF MOTION.

THE laws of motion form the foundation of all mechanical philosophy; and we cannot be too strict in the examination of them if we wish the superstructure to be either useful or durable. It is to be suspected that a want of due attention to this particular has rendered these laws, what they at present are, of little or no use in the purposes of life.

FIRST LAW OF MOTION.

THE First Law of Motion is thus defined by Sir Isaac Newton.

"Every body perseveres in a state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon."

This tendency of matter to persevere in its present state constitutes what is called the Inertia of matter. It gives to matter stability when at rest, and force when in motion. Though this property has not so much excited the attention or curiosity of philosophers as gravity, it is certainly equally mysterious and incomprehensible. That matter should continue in its present state without alteration, unless acted upon, is what we should readily conceive; but that it should present a powerful resistance to change is not less wonderful than that it should have a tendency to move. The one is a positive, the other a relative activity; and their causes seem equally beyond the reach of direct investigation.

We cannot leave this part of the subject without taking notice of a case which Sir Isaac Newton puts, to prove that motion may be both lost and gained; a case which, if granted, would entirely overturn this first law of motion. "If two equal balls," says he, "joined together by a slender wire, revolve with an uniform motion about their common centre of gravity, and that centre be carried uniformly forward in a right line in the plane of their circular motion, the sum of the motions of the two balls, as often as they are in a right line drawn from their common centre of gravity, will be greater than their motions when in a line perpendicular to that other." Now, this is in direct contradiction to the first law; for it supposes a change in the motions of the bodies, without any alteration in the forces impressed on them.

But the truth is, Sir Isaac's mistake lies in calculating the motions of the ball by the simple instead of the square of the velocities, which is their proper measure, as shall be shewn when we come to examine the second law. When this measure is adopted, it will clearly appear, that, in the above case, there is no motion either lost

or gained. Suppose, for instance, the balls each $=1$, moving about their common centre with a velocity of 5, and, at the same time, that common centre moving uniformly forward with a velocity of 5. Now, when they are in the right line of the motion of their common centre, they are each moving with a velocity of somewhat more than 7.071, the square of which, multiplied by 2, the sum of the balls, gives 100. Again, when both the balls are in the line perpendicular to that other, the one is at rest and the other is moving with a velocity of 10; the square of which, by 1, gives likewise 100. This is one proof, amongst numberless others, which we have in confutation of the measure of force adopted by Sir Isaac in his definition of the

SECOND LAW OF MOTION;—VIZ.

"THE alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

"If any force generates a motion, a double force will generate double the motion, a triple force triple the motion," &c.

To a person of plain common sense, unacquainted with philosophical controversy, it must appear truly astonishing that men professing themselves philosophers should be divided in opinion about whether the simple velocity, or its square, should be adopted as the measure of force in a moving body. This is a dispute that never could have arisen had not mechanical philosophy been cultivated more with a view to the amusement of the world, and the gratification of vanity, than to render essential service to mankind. If ever that science is to be cultivated with a view to real utility, or

with a view to serve the useful arts, it must be founded not upon the visionary basis of the phenomena that appear, but upon the more solid foundation of those powers of nature that are actually felt, and those material impressions that are actually made. Upon seeing, we may be induced to believe; but it is by feeling that we ascertain the truth. In all the useful arts, or wherever resistance is to be overcome, the effects produced by a moving body is uniformly as the square of the velocity. A ball with a double velocity will penetrate to four times the depth into a solid body; or, it will rise to four times the height; or, in short, to whatever kind of resistance it is applied, a body moving with a double velocity will produce four times the effect before its motion is entirely stopped. Even with regard to the planetary motions, to which the simple velocity is supposed to be the measure of force so well adapted, it is only by the absurd method of correcting one error by means of its opposite, that the reasoning is to be reconciled with the phenomena. For no sooner do we enter upon the central forces, than we find, that a double centrifugal velocity balances a quadruple power of gravity; but the power of gravity is a centripetal force. A centripetal force is in direct opposition to a centrifugal; and powers that balance one another must be equal: consequently, a double centrifugal velocity must possess a quadruple centrifugal force, otherwise it could not balance a quadruple power of gravity. It will afterwards appear, too, that the square of the velocity is the measure to be used in proving the equilibrium of the planetary system. Those who use the simple velocity as the proper measure, complain of the others that they do not consider that a double time is required with a double velocity to produce a quadruple effect; and a double force, acting a double time, they

say, should produce a quadruple effect. But what has time to do with it? Does time supply it with any new addition of force? Certainly not: It is merely by its own inherent power that it continues to act.

It is this adoption of the elements of pressure and time, instead of pressure and space, that has led mathematicians to this erroneous measure of the force of a moving body; and the superficial view they have taken of the phenomena attending the collision of unelastic bodies has tended to perpetuate the mistake. A body might be exposed for ages to the force of gravity, or any other pressure, without having any power communicated to it if opposed by some sufficiently resisting medium. It is only by the degree in which the body yields to this pressure, or the perpendicular space through which it is made to move by it, that we can estimate the effect produced upon it; or, it is by the perpendicular space through which a body in motion is capable of moving, and the resistance in every part of that space, that we estimate the power in that body. The practical engineer always employs the elements of pressure and space in calculating the power of his engines. If he is going to construct an engine for raising water, or any other ponderous matter, from the bottom of a mine, he estimates the power he must give to it, by the weight of matter, and the height to which it is to be raised; or, if he is going to apply a waterfall to a wheel for the turning of machinery, he reckons the power of his wheel by the quantity of water and the perpendicular descent; and, thus far, by this simple process, he is never disappointed in his calculations; whilst the mathematician, by a complicated circuitous process of reasoning, misleads both himself and the practical mechanic whom he means to instruct. This simple method of using pressure and

space as the elements of power, affords us the principle of *Impetus*, *Momentum*, or *Potential Velocity;* signifying the total effect which a body in motion is capable of producing before it is completely stopped—an instrument the most simple, the most powerful, and efficient, that has ever been employed in mechanical calculations, whether we reason from cause to effect, or from effect to cause. Its value will particularly appear in the facility with which it enables us to analyze the most complicated rotatory motions. When we come to apply this principle of momentum to practical cases however, allowances are always to be made for power destroyed by friction, collision, &c.

In the generation of motion, the pressure and space being the same, the effect will be uniform, whatever the quantity of matter; but the pressure and time being the same, the effect will depend on the quantity of matter moved.

Suppose a body to be allowed to descend freely by the power of gravity through a certain space, it will acquire a certain power, or momentum, according to the space through which it shall have descended. But if, instead of descending freely by itself, we suppose another body of equal weight to be attached to it, by means of a slender thread passing over a pulley, and lying upon a horizontal plane, without the smallest obstruction to its motion, the first body cannot descend without dragging this other along the plane with a velocity equal to its own. The sum of the forces impressed upon the two, through the medium of the suspended body, in descending through the same space, will be the same as that impressed upon it singly in the first case. Therefore, the force impressed upon each, and, consequently, the square of the final velocity of each, will be but one-half. But if the square of the

final velocity is one-half, the square of the time will be double; and the square of the whole time being double in the time that the first body unconnected with the other, descended through the whole space, now that it is connected with it, it will descend through but one-half; and the square of the velocity acquired in that time will be but one-fourth, which, multiplied by a double quantity of matter, gives but one-half the effect; wherefore, the pressure and the time being the same, the effect, or the potential velocity, impressed upon a body, will be inversely as the quantity of matter it contains; or, the time and the quantity of matter being the same, the effect will be as the square of the intensity of the pressure.

It does not appear that this has ever been taken notice of by any writer on the subject, that the pressure and the time being the same, the effect should depend upon the quantity of matter acted upon; although the fact may be observed in every day's experience. For instance, in the firing of a common gun, the powder exerts an equal pressure upon the gun backwards that it does upon the ball forwards; yet the effects are very different. If the impetus of the gun backwards were equal to that of the ball in the other direction, it would be impossible for a man to sustain the shock upon his shoulder. The same thing may be observed in firing large guns on board of a ship. If the recoil of the guns were equal to the impetus of the balls, the firing a very few on one side at a time would be sufficient to rend the vessel in pieces. There are numberless other facts of a somewhat similar nature in the collision of bodies, and which are extremely interesting, and of the highest importance in practical mechanics; but these belong to the third law of motion, which is thus defined by Sir Isaac Newton:

THIRD LAW OF MOTION.

"To every action there is always opposed an equal re-action; or the mutual action of two bodies upon each other are always equal and directed to contrary parts."

This third law of motion is to be exemplified principally in the collision of bodies. Let an unelastic body in motion come into collision with another equal to it at rest; the moving body, by its pressure on the other, will communicate to it a certain degree of motion; whilst the one which was at rest, by an equal counterpressure against the moving body, will destroy in it the same quantity of velocity; and they will both move forward with half the velocity of the first. Here the pressure and the time are the same upon both bodies; and the velocity destroyed in the one is equal to the velocity communicated to the other; but the potential velocity destroyed in the one is very different from that communicated to the other. The same pressure, when applied to resist a body in motion, destroys power much faster in that body than it would communicate it to another in urging it from a state of rest. The first may be compared to a body in motion ascending against the power of gravity; the other to one descending from a state of rest by the same power. In the time that the first will have lost one-half of its velocity, it will have risen to three-fourths of the height to which its velocity is capable of raising it; and, consequently, will have lost three-fourths of its power. In the same time the other body, descending from a state of rest, will have descended through but one-fourth of the space through which the other is capable of rising, and have acquired but one-fourth of its power. The sum of the forces in the two at that instant when their velocities

are the same is but one-half the original power of the ascending body. So it is with the bodies meeting in collision; the sum of the forces in the two, after collision, is but one-half of the force, or potential velocity, which the moving body possessed before collision. If the body at rest is to the moving, or impelling body, as two to one, the velocity destroyed in the moving body will be to that communicated to the other likewise, as two to one; and the two will move forward with one-third the velocity of the first; and the sum of the forces in the two, after collision, will be but one-third of the original power of the impelling body. In short, the power remaining in the two, after collision, is always to the previous power of the impelling body, as the matter in that body is to the matter contained in the two. But the velocity of the common centre of the two, or the quantity of matter multiplied by the simple velocity, continues uniformly the same after collision as before it; and it is this fact, which has been so long understood—whilst it was never suspected that any power was lost in the collision betwixt the two—that has been the cause of that measure of force, " the quantity of matter multiplied by the simple velocity," having been at first adopted, and so long retained by the great body of philosophers.

There is always a very sensible change takes place in the form, or in the cohesion of the parts, on the collision of non-elastic bodies, which ought to have led philosophers to suspect a destruction of power to produce this effect. But, instead of this, it has been even attempted to prove, that this change may be effected without any power being lost or destroyed. If any method could be discovered by which solid bodies might be penetrated, or the cohesion of their parts overcome, without any loss or expenditure of power, it would be a valuable

discovery indeed in practical mechanics. But there seems not the smallest reason for supposing that such a discovery will ever be made. On the contrary, it is uniformly found that the power lost is, *ceteris paribus*, directly as the effect thus produced. Smeaton* saw that power must be lost in the collision; but he did not perceive the true theory, or the manner in which it is lost. In the experiments which he made on the subject, he employed two bodies of equal weight, and he found the loss of power to be exactly one-half; and he concluded this to be the proportion lost in all cases of the collision of non-elastic bodies. Had he made trials, however, with bodies of different proportions, he would have found very different results.

But although the loss of power attending the collision of unelastic bodies has been unobserved by philosophers, or its theory misunderstood, the fact itself has had considerable influence in the practice of the mechanical arts. For instance, in the hewing of the softer species of stones, our artificers use the chisel and the mallet, notwithstanding the loss of power that takes place in the collision betwixt the mallet and the chisel; but in hewing millstones, or granite, or any of the other harder species for building, where greater force is required, it is found necessary to use a single implement of a different construction, that may be made to act immediately with its own impetus. In like manner, in boring rock, for the purpose of blasting it with gunpowder, where the bore to be made is small, and of course no great power is required, a sort of chisel, provincially called a *jumper*, which is wrought with a hammer, is used; but where the bore is to be larger, and of course more power required to make it, it is now common to use an

* Fundamental Experiments upon the Collision of Bodies.

instrument that operates without the medium of a hammer. This is made much heavier than the other instrument; and is raised and let fall with its own weight, and what additional impetus the workman can give to it in its descent. Thus the collision, and the consequent loss of power, which necessarily attend the use of the hammer, are avoided; and its efficiency is found to be considerably greater, although there are certain inconveniences attending the mode of working it.

There are some machines, as the engine for driving piles, that operate entirely by collision; and in these it seems impossible altogether to avoid a waste of power; but this loss may be much lessened by a due proportioning of the impelling body to that which is impelled. If the weight of the hammer, or ram of the pile-engine, for instance, is equal to that of the pile to be driven by it, one-half of the power expended will be lost, or will produce no effect in driving the pile into the ground; no matter what is the impetus of the ram, or what height it may be made to descend from; this is the proportion of the power that will be expended in vain. If the weight of the ram is one-half of that of the pile, two-thirds of the power will be lost. If it is one-third of the weight of the pile, three-fourths of the power will be lost. Or if we could suppose the ram to be only one-hundredth part the weight of the pile, such a ram might make a deep impression on the top of the pile, but it would have almost no effect in forcing it into the ground, although its impetus, or the square of its velocity, multiplied by its quantity of matter, should be the same as in the case of the heavier ram: the proportion of the power lost would be to the whole power as 100 to 101. On the other hand, if the weight of the ram is double that of the pile, one-third only of the power will be lost; if three times the weight of the pile, one-fourth

will be lost; and so forth in other proportions. The effect produced being always to the whole power of the ram, as the weight of the ram is to the sum of the weights of both it and the pile.

There are other machines or implements, again, the design of which is to alter the form of bodies, or to break the cohesion of their parts, as in the forging of metals, breaking of stones, &c. In these operations the great object is to spend the whole impetus of the hammer upon the body operated upon; which is to be done by presenting the greatest possible resistance to it. If the body to be operated upon does not itself possess sufficient inertia to arrest the whole motion of the hammer, it must be placed upon another that will have that effect; and hence the use of the anvil in the forging of metals. The weight and form of the hammer, again, must be regulated by the change intended to be produced upon the body to which it is to be applied. For instance, in beating out a piece of iron on an anvil, either in length or breadth, the force should be communicated as equally as possible through the whole mass; hence, the weight of the hammer should be great, compared with that of the iron, and its face plain. A heavy hammer will force the iron against the anvil; the re-action of which against the under side of the iron will be nearly equal to the action of the hammer upon the upper side; and, consequently, the effect will be nearly uniform throughout. On the contrary, if the weight of the hammer is small, compared with that of the iron, the inertia of the iron will arrest nearly the whole motion of the hammer, even although its impetus should be the same as in the former case. The collision and the effect upon the upper side of the iron will be greater; but there will be little action or re-action betwixt the under side

and the anvil; and hence the under side of the iron will be little affected.

Again, there is another kind of operation, as in the rivetting of nails, which requires only the part struck by the hammer to be affected. And here the procedure must be the reverse of the last. A heavy hammer would force back the nail from its place; but it would have little effect in flattening the part struck, which is the object of the operation: consequently, a light hammer with a sharp face is always used for this purpose, and is applied with a quick stroke.

In the breaking of large stones a heavy hammer must necessarily be used. A small hammer with the same impetus will not produce the same effect. The latter may shatter the stone more on the part struck, but it will not communicate the same shock through the body of the stone that the former will do, to split it or break it in pieces. And the reason is this: if the stone is very much heavier than the hammer, the motion of the hammer is arrested by the inertia of that side of the stone which is struck, so that the opposite side scarcely feels the shock at all; yet a light hammer is found to be more efficient than a large one in breaking a small stone, if the stone is lying on a loose heap, or has nothing but its own inertia to resist the impetus of the hammer. And this is easily accounted for on the preceding principles. If the stone is very light, compared with the hammer, it presents but little resistance to it; it flies away with nearly the original velocity of the hammer, and of course there is little of the impetus of the hammer spent in the collision betwixt the two. But if the weight of the hammer is small, compared with that of the stone, there is a greater proportion of its impetus spent in the collision. If the hammer and the stone are of the same weight,

then one-half of the impetus of the hammer is spent in the collision; the tendency of which is to break the stone in pieces. Yet, again, if a small stone is placed upon a very large one, or on some immoveable bed, the heavy hammer will be less efficient than the light one in breaking it; for, then, the whole impetus of the hammer will be spent in the collision, and, as we observed with regard to forging a piece of iron with a heavy hammer on an anvil, the shock becomes more equably communicated through the whole mass; the re-action from below being nearly equal to the action of the hammer from above; and hence in this way the stone will be reduced into a greater number of pieces. It has accordingly been found, that, where it is wished to break stones very small, as the modern system of road-making requires, a man can perform much more work by this method than in the former way of using small hammers, and breaking the stones upon a loose heap. This method is attended, however, with one inconvenience, viz. it reduces much of the stones to a powder, which is unsuitable for road-making. But this may be remedied by afterwards sifting them.

The theory of the operation of the tools used by artificers and labourers, is altogether a curious and interesting subject, and worthy the attention of mechanical philosophers. But they have hitherto been prevented from entering much upon it from their erroneous ideas of the laws of motion, and of the nature of the relation of cause and effect in the mechanical action of bodies on one another.

In the collision of elastic bodies the result is very different from what takes place in the case of those that are unelastic. If one elastic body is impelled against another at rest, the consequence will be the same in the first part of the process, or during the

yielding of the parts, as if the bodies were unelastic. If the two bodies are equal, the first will have communicated one-half of its velocity to the other, whilst it itself will be as much retarded. But the action does not stop here: for the parts have a tendency to resume their original form; and in the distending of the springs the velocity of the impelling body will be extinguished, and that of the other will be doubled, or become equal to the velocity of the first before collision. In like manner, if one body is impelled against another of double weight, it will communicate one-third of its velocity to it in the collapsing, and another third in the distending of the springs; and the second body will move forward with two-thirds of the velocity, or eight-ninths of the power of the first; and the first will recoil with one-third the velocity, or one-ninth of its original power; and so on in the collision of bodies of other proportions.

In the collision of elastic bodies there is no power either lost or gained. If either of the bodies is elastic, the consequences are the same as if both of them were so. If both of them are imperfectly elastic, the power lost is in proportion as the yielding parts of the one which is most elastic fail to recover their original form.

It is hoped we may now be enabled to proceed to the the investigation of practical subjects by a way, not only shorter and more compendious, but that will likewise give us greater certainty in the results, than that which has hitherto been pursued by philosophers. The above discovery which we have made with regard to the application of mechanic power, viz. that the effect produced in the action of bodies on one another depends not solely on the action employed, but likewise on the re-action of the matter acted upon, although, at first sight, it may appear of very ordinary importance, is

one which, when followed out in all its various bearings in the different branches of science, cannot fail in a great measure to revolutionize every department of philosophy, moral as well as physical, inasmuch as it leads us to resolve all the phenomena of nature into the principles of action and re-action, instead of attributing them, as at present, to the absolute properties of matter or of mind. If this has hitherto passed unobserved by philosophers, and if they have fallen into such inaccuracies in a subject where all the elements are so evident to our senses as that of material motion, we may surely be allowed a rational scepticism with regard to the doctrines in some of the other sciences, where the truth is necessarily more difficult of access.

CHAPTER II.

THE PERCUSSION AND RESISTANCE OF FLUIDS.

THE same mode of computing the mechanic power impressed upon a body, viz. by pressure and space, and the same measure of the power of a body in motion, the quantity of matter and the square of the velocity, which we have adopted for the solution of the phenomena of the motion of solid bodies, will be found no less applicable to the action and resistance of fluids.

We shall first consider fluids as applied to communicate motion to machines; this forming the most simple part of the theory of the motion of fluids.

Before the discovery and improvement of the steam-engine, water was considered the cheapest, the most powerful, the most manageable, and the steadiest of all the means employed for the turning of machinery. And although the steam-engine has considerably lessened the importance of water, the latter is still always employed wherever a sufficient and regular supply of it, with a suitable fall, can be conveniently had. The application of water to the moving of machines has of course always formed, and continues to form, a very important branch of mechanics. It has commanded a good deal of the attention of our philosophers and mathematicians; but what is remarkable, there still are but few points of the theory with regard to which they

are perfectly agreed. It is fortunate, however, that our practical men have found fewer difficulties. By attentive observation, wherever any errors have been committed in practice these have generally been detected and corrected; and they have thus certainly brought the art to very great perfection, and settled upon general rules by which they seldom err to any great extent; whilst our philosophers, in the mean time, have been unable to follow them up and to trace the true theory.

OF OVERSHOT WHEELS.

Of all the different ways of applying water to turn machinery the overshot wheel is the most powerful in practice, and the most simple in its theory. After the water is brought upon the wheel, it descends along with it with an uniform unaccelerated motion, and consequently presses upon the wheel with its whole weight through every part of its descent; and is calculated to raise a weight equal to the weight of the water, and to an equal height, making an allowance only for the loss of power by friction. In bringing the water upon the wheel, there is a certain portion of its descent necessarily set apart for giving the water a proper velocity before it comes upon the wheel. This may be considered as so much perpendicular space lost in point of power; but it need never be great—from nine inches to a foot, or fifteen inches, is sufficient for a wheel of any size. In bringing the water upon the wheel, a velocity should be given to it very little more than that of the wheel itself. Whatever velocity is given to it more than this, produces comparatively little effect; as that portion of it acts by impulsion, or upon the principle of the undershot wheel, which has not half the power of the overshot.

It has been a question amongst mathematicians,

What is the most proper velocity for an overshot wheel, in order that it may produce the greatest effect? Smeaton, whose authority has been much looked up to, says, three feet per second. But this is much less than what is generally allowed by practical men, which runs from four to seven feet per second, according to circumstances. The only loss that can arise from giving the wheel too great a velocity must be from one or other of the following causes: viz. the perpendicular height that is sacrificed to give to the water the velocity of the wheel before it comes upon it, which is a very small matter, as the difference betwixt three feet per second, and seven feet, requires only about seven and a half inches of perpendicular height; or there may be a difficulty in throwing the water into the buckets; or it may be thrown out again by the centrifugal force, if the velocity of the wheel is too great. There need be no great loss, however, from either of these causes, if things are properly constructed, when the diameter of the wheel is not less than fourteen feet, and the velocity of its circumference not more than seven feet per second. But Mr Smeaton seems to have imagined, that, in giving the wheel a great velocity, there is a loss of power independent of these causes. "Though the product," says he, "made by multiplying the number of cubic inches of water, acting in the wheel at once by its velocity, will be the same in all cases; yet, as each cubic inch, when the velocity is *greater*, does not press so much upon the bucket as when it is *less*, the power of the water to produce effects will be greater in the less velocity than in the greater; and hence we are led to this general rule, That, *cæteris paribus*, the less the velocity of the wheel, the greater will be the effect thereof."

Now, this is quite an erroneous idea of Smeaton's.

It is an established law in mechanics, that when a body is descending by gravitation, whatever velocity it may have acquired, the power of gravity will still continue to press it downward with the same force, when it has acquired the velocity of a thousand feet per second, as if it had only a velocity of three. The water must have a velocity at least equal to that of the wheel before it is thrown upon it; and unless it is thrown out again by the centrifugal force, it will continue, during its whole descent, to press upon it with its whole weight, whether it moves fast or slow.

But though Smeaton recommends the velocity of an overshot wheel to be so low as three feet per second, yet he acknowledges, that "it is an advantage, in practice, that the velocity should not be diminished farther than what will produce some solid advantage in point of power; because, *cæteris paribus*, as the motion is slower, the buckets must be made larger; and the wheel being more loaded with water, the stress upon every part will be increased in proportion."

There is another disadvantage attending a very slow motion: When the resistance is not perfectly uniform, the motion is not so steady as with a greater velocity. If the resistance happens at any instant to be lessened, or removed altogether, the wheel is more apt to start into a greater velocity; or if it should meet with any temporary increase of resistance, its power to overcome it is much less. The water-wheel acts not only in turning the other parts of the machine, but likewise as a fly-wheel in giving regularity to their motions; and its power, in this latter respect is as its weight and the square of its velocity. When the work in which a water-wheel is employed does not present an uniform resistance, it is of importance that the wheel itself should have a considerable weight to produce a regular

motion. In separating corn from the straw, for instance, the resistance is not so uniform as in grinding it into flour; therefore, the former process, *cæteris paribus*, would require a heavier wheel, with a somewhat greater velocity. When Mr Meikle, the inventor of the thrashing-mill, first employed water to turn these machines, he found their motion frequently so very irregular, that he was like to give the preference to animal power for that purpose, till he thought of increasing the weight of the water-wheel, which had the desired effect of equilizing the motion. When thrashing-machines are turned with overshot water-wheels, these are generally made to move at the rate of six or seven feet per second. However high the name of Smeaton may stand as a practical engineer, it is well known that there were flour mills erected under his directions, in which it was afterwards found necessary to increase the velocity of the water-wheels.

OF UNDERSHOT WHEELS.

WATER-WHEELS are generally considered by practical men to consist of three kinds, viz. *overshot wheels*, which receive the water on their top in buckets, and are carried round by its weight; secondly, *breast wheels*, which likewise receive the water in buckets, but at some lower point than the top—on these the water acts likewise by its weight; thirdly, the *undershot*—here the water is received on float-boards, at some point much below the centre of the wheel, and where it is generally supposed to act by impulse; but even here, too, it commonly acts much more by its weight descending with the wheel than by impulse.

Scientific men, on the other hand, generally divide water-wheels into two kinds, as acting on two distinct

principles; viz. *overshot*, on which the water acts entirely by its weight, and *undershot*, where it acts entirely by impulse. It is this latter kind which we are now going to consider; and which has been the subject of more discussion among the learned than even the overshot, though greatly inferior to it in practice in point of power; but what may seem remarkable is, that authors are yet by no means agreed, either as to the effect it is capable of producing, or the velocity it should have, as compared with that of the water, in order that it may produce the greatest effect. Nay, not only are the learned not agreed on these things, but there is even a difference of opinion among them as to the force with which a vein of water, spouting from a round hole in the side of a vessel, presses upon a plane directly opposed to it. The more general opinion is, that the pressure of the vein, flowing uniformly, ought to be equal to the weight of a cylinder of water whose bore is equal to the hole through which the water flows, and its height equal to the height of the water in the vessel above the hole. But this is a mere vague assumption, without any rational foundation. It is evidently an erroneous idea to suppose that the area of the hole affords the measure of the thickness, or the transverse section of the spouting vein; for at a very short distance from the hole the vein becomes contracted in the ratio of nearly 64 to 100; and it is after this contraction that the water moves with the velocity due to the height from whence it has descended. It is therefore the contracted vein that should be adopted in computing the transverse section of the vein of water issuing from the vessel. Neither is it the height of the fluid in the vessel, but the double of that height that denotes the velocity—and consequently the length—of the vein striking the plane in

the time occupied by the fluid in descending from the top of the vessel. For instance, if the height of the water in the vessel is sixteen feet, it will fall through that space in nearly a second of time, and acquire a velocity of about thirty-two feet per second. But, perhaps, the best way to elucidate the pressure of a vein or column of water against a plane opposed to its motion, will be to suppose the column of water to be at rest, and the plane to move against it.

When motion is communicated to a solid body by an uniform pressure acting simultaneously on the whole body, equal increments of velocity are generated in equal times; and the pressure necessary to produce any given velocity in a given time, is as that velocity and the quantity of matter. But if we are to communicate motion to a column of fluid by a plane moving against it in the direction of its length with an uniform velocity, the case is somewhat different: here it is the quantity of matter put in motion that increases uniformly with the time. The effect, however, as to the quantity of resistance is the same; for the increments of the quantity of matter must have the same effect in producing resistance, as the increments of velocity in the case of the solid body. But it requires an uniform pressure equal to its weight to communicate to the solid body a velocity of thirty-two feet in a second of time; so, to communicate to a vein of water, by means of a plane moving against it with an uniform pressure, that plane must be made to move uniformly at the rate of thirty-two feet per second, and will sustain a resistance equal to the weight of a column of water of the same transverse section, and thirty-two feet in height; or, as action is equal to reaction, if the vein of water is made to move against a fixed plane, it must produce against it the same pressure.

But in this way of communicating motion by collision to a vein of water thirty-two feet in length, the pressure must be continued over the whole space of thirty-two feet; whereas, in communicating motion to a solid body, containing the same quantity of matter, by an uniform pressure and with an accelerated velocity, the same pressure has only to travel over sixteen feet; hence, in this way of communicating motion by collision, we see that it requires the expenditure of twice the mechanic power to produce the same effect; or, in other words, that one-half of the power is lost. In a former part of our inquiry, we found that power is always lost in the collision of unelastic solid bodies; so here, too, it is only when the fluid is unelastic that this loss of power takes place. If the water were elastic, so as to rebound from the plane with perfect elasticity, it would then, indeed, require twice the pressure along the space of thirty-two feet; but this double pressure, by producing a double velocity, would produce a quadruple power. Or if the same pressure is continued over only sixteen feet against this column of elastic fluid, it will generate in it, the same velocity as if continued over the whole thirty-two feet against a column of unelastic fluid.

Agreeable to the above view, Dan. Bernoulli has determined the force with which a vein of water, spouting from a round hole in the side of a vessel, presses against a plane opposed to its motion, to be equal to the weight of a cylinder, whose transverse section is equal to that of the contracted vein, and whose height is double that of the water in the vessel above the hole. We see, however, that this computation of Bernoulli's can be true only on the supposition that water is perfectly unelastic; but, it is well known that water possesses a considerable degree of elasticity; its force must there-

fore be considerably increased by the action of its recoil from the plane; and, accordingly, though we are told by Bernoulli that he found his theoretical computation proved by experiment, yet other experiments * have made it one-fourth greater.

When a vein of water is made to strike against a plane in order to produce some mechanical effect, it is evident that the plane must not remain at rest; for then it could not be performing any work whatever. Neither must it move so fast as the water; for then the water could not strike it with any force. It must therefore move with some intermediate velocity; but as to what that is, our mathematicians are by no means agreed. It has generally been determined that the plane should move with one-third of the velocity of the water; but this has not been acquiesced in by some mathematicians; neither is it borne out by experience, when applied to a system of planes, as in the case of an undershot-wheel, though it is perfectly correct when applied to a single plane. This will require a particular consideration.

The force with which a vein of water strikes against a fixed plane is as the square of its velocity; or as the quantity of water, striking the plane in a given time, multiplied by the velocity of that water. Again, if the plane against which the water strikes is not fixed, but moves in the direction of the stream, and somewhat slower, the pressure upon it will then be as the square of the relative velocity of the water; and the mechanical effect which such a plane is capable of producing, will be greatest when the pressure upon the plane, multiplied by the velocity of the plane, is a maximum; that is, when it moves with one-third of the velocity of the water: for then the relative velocity will

* By the author.

be two-thirds of the absolute velocity, and 2 is that part of 3, whose square, multiplied by the remainder, gives the greatest product; and the effect will then be eight twenty-sevenths of the power expended. If this were the principle upon which water acted on an undershot wheel, its effect would, of course, be a maximum when moving at the same rate, or with one-third of the velocity of the water. But this is not the principle on which an undershot wheel is acted upon. A plane, or a single float-board, moving in the direction of the stream, with one-third of its velocity, in any given time, would be overtaken or struck by only two-thirds of the stream, issuing from the aperture or fountain-head, in the same time. But an undershot wheel has more than one float: it is formed with a set of floats, which, as it revolves, intercept the whole stream as it issues from the aperture; and is, therefore, struck with the same quantity of water, whether it moves fast or slow.

Mr Waring and others, adverting to this circumstance, have determined, that the quantity and absolute velocity of the water being the same, the pressure of the stream upon the wheel, is, simply, as the relative velocity; and that it produces a maximum effect when the wheel is moving with one-half the velocity of the water: 1 being that part of 2 which, multiplied by the remainder, gives the greatest product. This gives a greater effect than the theory of the forementioned mathematicians, who would have the wheel to move with only one-third of the velocity of the water; as it gives both a greater pressure upon the wheel, and, likewise, a greater velocity to it. It makes the effect equal to one-half the power expended; or, if applied to that purpose, it returns one-half the quantity of water to the height from whence it fell. If the wheel is impelled, for instance, by a vein of water moving with the velocity of thirty-two feet per second, and

the wheel itself moving with a velocity of sixteen feet, it will be pressed with a force equal to the weight of a column of water whose transverse section is equal to that of the impelling vein, and whose height is sixteen feet; and it will be capable of raising that quantity of water, in a second of time, to the height of sixteen feet; being the same height as that from whence the impelling vein had descended; or exactly one-half the quantity that had descended in the impelling vein in the same time, which will be equal to a column of the same transverse section, but thirty-two feet in height. This is certainly the true theory of the undershot wheel, however far the effect may come short of this in practice, from the difficulty, or the almost impossibility, of making the jet any thing like complete, and from other causes.

It must be acknowledged, that this theory seems to correspond very ill with the experiments of Smeaton. But though these experiments have been quoted by most authors who have written on the subject since his time, yet they have never undergone that strict scrutiny which the subject requires; and which would certainly have led to conclusions very different from those inferred from them, both by Smeaton himself and others, with regard to the velocity which the wheel should have, compared with that of the water, as well as the ratio which the effect will have to the power expended. In proof of this, we shall here examine a

"*Specimen of a Set of Experiments on an Undershot Wheel, applied to raise a weight.* *

"The water above the floor of the sluice, 30 inches.
Strokes of the pump, for filling the cistern,
in a minute, - - - - - 39½

* Experimental Enquiry, page 10.

The head raised by twelve strokes, - 21 inches
The wheel raised the empty scale, and made
turns in a minute, - - - - 80
With a counter-weight of 1 lb 8 oz. * it made 85
Ditto, tried with water, - - - 86

No.	Weight. lb.	oz.	Turns in a minute.	Product.
1	4	0	45	180
2	5	0	42	210
3	6	0	36¼	217½
4	7	0	33¾	236¼
5	8	0	30	240 max.
6	9	0	26½	238
7	10	0	22	220
8	11	0	16	181½
9	12	0† ceased working.		

" Counter-weight for 30 turns, without water, 2 oz. in the scale.

" N. B.—The area of the head was 105.8 square inches. Weight of the empty scale and pulley, 10 oz. Circumference of the cylinder, or axle of the wheel, round which was wound the line for raising the scale, 9 inches. Circumference of the water wheel, 75 ditto.

" *Reduction of the above Set of Experiments.*

" The circumference of the wheel, 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; one-sixtieth of which will be

* N. B.—The counter-weight of 1 lb. 8 oz., together with the weight of the scale, 10 oz., are here supposed to balance the friction.

† " When the wheel moves so slow as not to rid the water so fast as supplied by the sluice, the accumulated water falls back upon the aperture, and the wheel immediately ceases moving."

the velocity in a second, equal to 107.5 inches, or 8.96 feet, which is due to a head of 15 inches; * and this we call the *virtual* or effective head.

" The area of the head being 105.8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal .579 of the ounce avoirdupois, gives 61.26 ounces for the weight of as much water as is contained in the head, upon 1 inch in depth—one-sixteenth of which is 3.83 pounds; this multiplied by the depth, 21 inches, gives 80.43 pounds for the value of 12 strokes; and, by proportion, 39½ (the number made in a minute) will give 264.7 pounds, the weight of water expended in a minute.

" Now, as 264.7 pounds of water may be considered as having descended through a space of 15 inches in a minute, the product of these two numbers, 3970, will express the *power* of the water to produce mechanical effects; which were as follows:

" The velocity of the wheel at the *maximum*, as appears above, was 30 turns a minute; which multiplied by 9 inches, the circumference of the cylinder, makes 270 inches; but as the scale was hung by a pulley and double line, the weight was only raised the half of this, viz, 135 inches:

	lb.	oz.
" The weight in the scale at the maximum,	8	0
Weight of the scale and pulley, - -	0	10
Counter-weight, scale and pulley, - -	0	12
Sum of the resistance, - - - -	9	6

" Or 9.375 lb.

* " This is determined upon the common maxim of Hydrostatics, that the velocity of spouting water is equal to the velocity that a heavy body would acquire in falling from the height of the reservoir; and is proved by the rising of jets to the height of their reservoirs nearly."

"Now, as 9.375 pounds is raised 135 inches, these two numbers being multiplied together, the product is 1266, which expresses the effect produced at a maximum; so that the proportion of the *power* to the *effect* is as 3970 : 1266, or as 10 : 3.18."

The first observation that we would make on the above set of experiments is, that when we compare the number of turns in No. 3, viz. 36¼, with the number of turns in the one that precedes, and in the other that follows it, viz. 42 and 33¾, we do not find that regular progression which prevails amongst the rest. There is evidently a mistake in it. But if we substitute 38 for 36¼, we will find the progression pretty regular throughout the whole.

In the next place, in putting down the weight raised, Smeaton has not included the weight of the scale, which is evidently as much a part of the weight raised as that in the scale. In a theoretical point of view, the allowance for friction should likewise have been included, or indeed even for practical purposes: for friction does not form an invariable quantity; there being several means by which it may be indefinitely reduced; and it is seldom or never the same in two different machines. He afterwards, indeed, takes in both these in estimating the effects produced; but it was equally necessary that he should have done so here in determining the velocity that produces a maximum. With these corrections, we shall insert the above statement below; to which we shall likewise add the relative velocities of the water in turns of the wheel per minute. We shall likewise add the weights that these relative velocities *ought* to have raised according to theory on the above supposition, that the force of a stream of water, striking a fixed plane, is equal to the weight of a column whose

transverse section is equal to that of the stream, and whose height is equal to double the altitude due to the real velocity; which altitude is termed by Smeaton, "the *virtual* or *effective* head." In these experiments, from the weight of water expended, 264.7 pounds avoirdupois, at the rate of the decimal .579 of an ounce to the cubic inch, we have 7318.4 for the number of cubic inches; which, divided by the velocity, 6450 inches per minute, gives 1.1346 for the transverse section of the spouting stream; this multiplied by 30, or twice the virtual or effective head, gives in round numbers 34, for the cubic inches contained in the column; the weight of which is 19.6 oz. But in raising the weight, this acts upon a lever of considerable power. The circumference of the wheel on which the water acts is seventy-five inches; whilst that of the cylinder, which constitutes the axle of the wheel, and round which was wound the line to which the scale was attached, is only nine inches; being in the ratio of somewhat more than eight to one. But as the water does not act on the very extreme circumference of the wheel, but somewhat within it, whilst the power of the line extends half its own thickness beyond the circumference of the cylinder, we may suppose the water to be acting with a power equal to eight times its own weight. And, again, as the scale was hung by a pulley and double line, the power was thereby doubled: consequently, the power of the water upon the wheel should have balanced sixteen times its own weight in the scale, or 19 ℔ 9½ oz. This is the power of the vein of water acting with its absolute velocity against a fixed plane, or against the wheel in a state of rest; and from hence we can easily find the power corresponding to the several relative velocities, which will stand as under in the last column:—

No.	Weight. lb.	Weight. oz.	Turns in a minute.	Product.	Relative velocity.	Weight by theory. lb.	Weight by theory. oz.
1	5	12	45	258.75	41	9	5
2	6	$10\frac{4}{5}$	42	280.3	44	10	0
3	7	$9\frac{1}{5}$	38	287.7 max.	48	10	15
4	8	$7\frac{1}{2}$	$33\frac{3}{4}$	285.8	$52\frac{1}{4}$	11	$14\frac{1}{3}$
5	9	6	30	281.2	56	12	12
6	10	$4\frac{3}{5}$	$26\frac{1}{2}$	272.6	$59\frac{1}{2}$	13	9
7	11	$2\frac{4}{5}$	22	255.8	64	14	$9\frac{1}{3}$
8	12	$0\frac{3}{5}$	$16\frac{1}{2}$	198.6	$69\frac{1}{2}$	15	$13\frac{1}{2}$
9	12	10 ceased working.			86	19	$9\frac{1}{2}$

These corrections present us with a very different view of the subject in several respects; and must lead us to draw inferences from those experiments very different from those Mr Smeaton himself has done. We now find the maximum effect to be at thirty-eight turns per minute, instead of thirty, as in the former statement. Thirty corresponds with very little more than one-third of the absolute velocity of the water, whilst thirty-eight approaches to much nearer one-half. These corrections show us, too, that a much greater latitude may be used with regard to the velocity of the wheel, compared with that of the water, than Mr Smeaton's statement supposes, without materially influencing the effect: from twenty-six and a half turns to forty-two, does not make a difference of one-nineteenth part.

We also find that Mr Smeaton entertained a very erroneous idea in supposing that the impulse of the water, in the case of a maximum, in these experiments, was the double of that assigned by theory—that it was nearly equal to the whole column; when, in fact, it was not equal to four-ninths of that column, as determined by the theory to which he alludes: the

impulse on the floats only, balanced in the scale 7 ℔ 9 oz., when the weight of the whole column should have balanced 19 ℔ 9 oz. Mr Smeaton estimates the force of the whole column, when striking with its absolute velocity against the wheel at rest, by the load of 12 ℔ 10 oz., with which it ceased moving. But it was certainly not altogether this load that stopped its motion, it must have been a good deal aided by the accumulation of water in the conduit, breaking the force of the impelling stream. Thus, not only was Mr Smeaton wrong in supposing the effect to be so much greater than that assigned by theory, whilst it was positively less; but this deficiency was certainly principally owing to that very circumstance to which he ascribes the supposed double effect, viz. to the wheel moving in a conduit, whereby the water, after having struck the wheel, was prevented from immediately escaping.

In a former part of this inquiry, we found that power is necessarily lost in collision; and that the more matter intervening betwixt the impelling body and that operated upon, the more power is destroyed in overcoming the inertia of that matter. So when the water, after having struck the floats, is not allowed immediately to escape, it must necessarily break the force of that which follows. It is impossible, indeed, to conceive how any advantage can be derived from retaining the water after it has struck the floats: its power is then spent, and it can produce no farther effect in that direction. Smeaton quotes a passage from Euler, from whence it appears, that that author entertained an opinion similar to his own. Euler supposes, that the water, after striking the floats, moves towards the sides, where it exerts a particular force by which the effect of impulsion is increased. But whatever force the water may

in this way exert against the floats, it must have at least an equal effect in breaking the force of that which is following, by returning against it; whilst its very inertia must always be an obstruction to the impelling stream. This is farther confirmed by examining the above corrected statement: when the wheel makes forty-two turns per minute, the weight raised is to that which ought to have been raised, according to theory, nearly as 2 to 3; but as the velocity of the wheel decreases, and the relative velocity of the water increases, the experimented weight, and the weight by theory, approach nearer to each other. Now, as the velocity of the wheel diminishes, the quantity of retarded water indeed increases; but the power of the impelling stream to push aside this water, being as the square of the relative velocity, increases still faster, nearly as far as these experiments go, when the proportions begin to take a different turn. By the time the number of turns of the wheel is reduced to 16½ per minute, the retarded water accumulates faster than the square of the relative velocity of the impelling stream increases; and by the farther reduction of the velocity of the wheel, even by a few turns per minute, the retarded water accumulates so fast, that the impelling stream is unable to strike through it with sufficient force to raise 12 ℔ 10 oz.; when, had the wheel been clear, and the water had free room to escape on every side, it ought to have raised 19 ℔ 9½ oz.; or, according to the later experiments* on the force of a spouting vein, formerly mentioned, even a fourth part more.

There is another cause which must likewise tend to diminish the effect: before any given float comes to

* By the author.

be at perfect right angles with the direction of the stream, another float, whose obliquity is still considerable, is intervening, and receives part of the stroke.

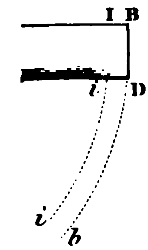
It is remarkable, that Mr Smeaton takes no particular notice of the very great difference betwixt the virtual or effective head, and the actual height of the water in the cistern. In the above set of experiments, it was exactly one-half. The virtual or effective head, deduced from the velocity of the water, he makes only fifteen inches, whilst the real height from which the water descended in the cistern was thirty; still he could not raise a weight equal to one-third of the water expended, even calculated in this way; and after taking in, too, an allowance for the power remaining in the water after it had left the wheel, which has nothing to do with the effect which an undershot wheel can produce. But when we compute the expenditure by the actual height from which the water descended, the effect was not one-sixth of the power expended. This puts the inefficiency of the undershot wheel (we mean a wheel constructed to act strictly on the undershot principle) in the strongest point of view; and shows the impropriety of using it where the greatest effect is required.

But, still, after all that has been said and written on the theory of undershot wheels, it is more a subject of curiosity than real utility; for, though a great proportion of our water-wheels are constructed on the form of undershot, and from the water acting only on the under part of them, they may with propriety enough be termed undershot wheels; yet, on by far the greater part of them, the water acts chiefly on the overshot principle—that is to say, it acts more by its weight descending with the wheel, than by its impulse after its descent.

The perfection attained both in carpentry and masonry, by our modern artists, has enabled them so to accommodate the conduit, or race of the wheel, to the floats, that but a small proportion of the water can escape between them. The floats are at the same time supplied with a board which prevents the water from dashing over into the interior of the wheel; so that nearly the whole of the water descends upon the floats, on the same principle as in the buckets of an overshot wheel; and, in proportion to the height of the fall, with nearly the same effect. Floats have indeed one advantage over buckets, that when the water is leaving the wheel, it is more easily disengaged from them. When the fall is great, however, the bucketed wheel is always to be preferred.

When a wheel with floats is constructed, as here described, to act with the weight of the water, the water should be received upon it at as high a point as possible. It should merely be allowed a previous descent sufficient to give it a velocity very little more than that of the wheel.

There are many cases where the quantity of water, or the fall, is greater than is requisite for the work that is to be performed; and where it is of greater consequence to save expense in the construction of the machine than to obtain the greatest effect from the water. The wheel with floats is then made use of; and the water is made to descend upon an inclined plane, and to act on the wheel by impulse. A greater velocity can thereby be given to the wheel than if it acted upon the overshot principle, and the other parts of the machine are thereby rendered more simple.



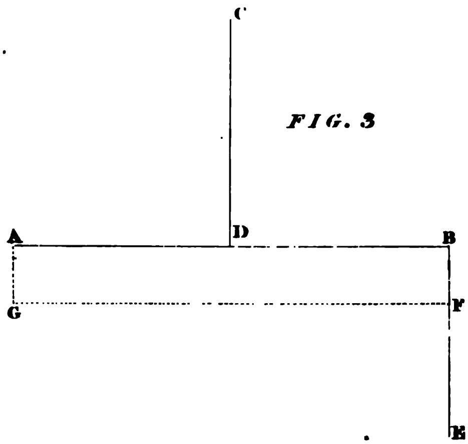


FIG. 3

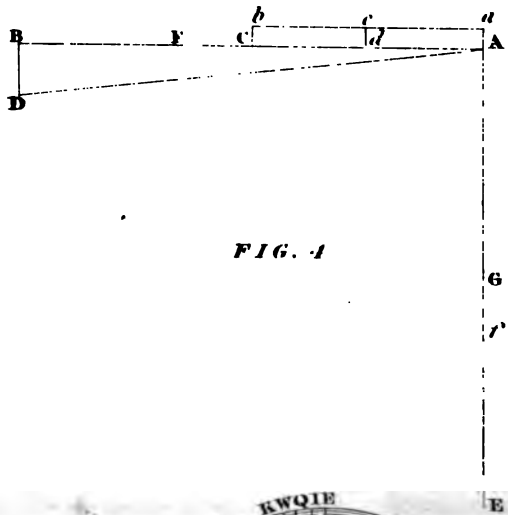
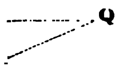
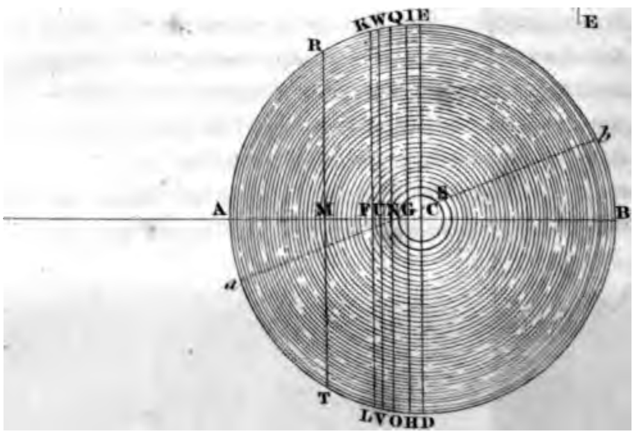


FIG. 4





BARKER'S MILL.

THE machine known by this name acts upon a principle different from any of those above-mentioned. It is commonly said to act by the reaction of the water; but this is not very correct. It is moved by the unequal pressure of the water on its opposite sides, in consequence of the escape of the water by an aperture on one side; but the power by which it is impelled, by no means corresponds with the force by which the column issuing from it is urged into motion. It cannot, therefore, be said to act by reaction.

This mill has afforded a good deal of discussion amongst mechanical philosophers; but they are still by no means agreed as to its theory, or the effect it is capable of producing. It is hoped the following will be found a more satisfactory and correct view of the subject, than any that has hitherto been given. We introduce this mill here, not from any high opinion of its merits as a practical machine, but from the notice that has been taken of it by philosophers and mathematicians; many of whom would still press it upon the world as a valuable discovery. It is, certainly, not altogether unfit for practice; but it does not possess that power which some would ascribe to it, nor is it so easily constructed as seems generally to be imagined.

This machine is represented by Fig. 1., in which A is an upright tube, BC, an horizontal tube, fixed across the bottom of it, and communicating with it internally, and having two apertures, *d* and *e*, near the extremities, but on opposite sides. FG is an upright spindle, fixed into the upright tube, by two cross-bars at *f*; so that, if the horizontal and upright tubes be turned round, the spindle, FG, will be turned also. On the

top of the spindle is fixed a mill-stone, or some other piece of machinery, according to the purpose for which it is intended. H is a pipe or channel that brings the water upon the tube.

It is evident, from inspection, that when the whole is filled with water, and the two apertures, *d* and *e*, are shut, there can be no motion; but when these are opened, the two ends of the horizontal tube in which they are situated, will each be relieved from a pressure equal to the weight of a column of water, whose transverse section is equal to the area of the aperture, and whose height is equal to that of the water in the upright tube: each end will of course move backward with a force corresponding to this. This will be the force with which the machine will begin to move from a state of rest. But after it is put in motion, the water in the horizontal tube acquires a centrifugal force, whereby the pressure of the water is increased, not only against the ends, but likewise its sides, especially towards its extremities. On the other hand, as the water enters the horizontal tube, and passes along towards its extremities, there is an angular motion communicated to that water by the tube; from whence arises a reaction, which retards the motion of the machine.

The theory of this machine will be better understood by the bird's-eye view of it, Fig. 2., in which ABDE represents the horizontal tube, divided into two equal parts, AFGE and BDGF, commonly termed the arms of the machine. The circle FG, round the centre C, represents the upright tube; and H, I, the two apertures from whence the water issues. We shall examine the theory of the arm AFGE: the same reasoning that applies to it will likewise apply to the other.

When the machine is at rest, and filled with water, and the aperture at H first opened, the water will issue from it with the velocity due to the height of the upright tube. But after it has begun to move, all the water in the tube will have acquired a centrifugal tendency; beginning with nothing at the centre C, and increasing in arithmetical progression, from that, to the extremity of the arm: consequently, the greater the velocity of the arm, the greater is the pressure of the wáter in it, both against the end of the arm, and likewise against its sides; and the greater is that part of the pressure, from which the side, EG, is relieved, by opening the aperture H; and the greater the force with which the opposite side will be pressed backward in the direction Aa.

On the other hand, from the water that is continually passing along the arm, from the centre to the aperture at H, there arises a great resistance. In leaving C, the water has a tendency, from the pressure of the column in the upright tube, to move from that centre towards the extremity of the arm; but it has none to move in the direction Aa: this latter motion is communicated to it entirely by the motion of the arm; and the reaction falls against the side EG. The quantity of this reaction depends upon the quantity of water that passes along the arm, from C to H, in a given time; and the velocity that is given to it in the direction Aa. This quantity of water, again, is regulated by the transverse section of the contracted vein, formed at a little distance from the aperture, and its velocity. But the whole resistance to the motion of the arm is the double of this: for it is evident, that when the water reacts on the side EG, it must fall back in an equal degree from the side AF; and the

whole resistance is the difference of the pressures on the two sides. We shall here put a hypothetical case to find, what is the greatest velocity which this mill can acquire in given circumstances.

Let the height of the upright tube be equal to the space through which a body falls by the force of gravity, at the surface of the earth, in a second of time; which, for the sake of even numbers, we shall suppose sixteen feet; and the length of the arm, from C to the aperture H, equal to the space through which the body moves, in the same time with the velocity which it acquires in falling through that space, or thirty-two feet.

Let the upright tube and the arm be filled with water, and the aperture at H be opened. The water will begin to issue from it, (on the supposition of the jet being complete,) with the velocity of thirty-two feet per second; and the extremity of the arm will move backward in the direction A*a*. When it begins to move, it is pressed with a force equal to a column of water whose height is sixteen feet, and whose transverse section is equal to the area of the aperture, acting upon the side AF, at a point opposite the centre of the aperture H. Let the arm be accelerated till, at the aperture, it moves with an uniform velocity of thirty-two feet per second. All the water in the arm will now have acquired a centrifugal tendency; the force of which will begin with nothing at the centre C, increasing directly as the distance from it, till at the aperture it is equal to the force of gravity. The whole centrifugal force of the thirty-two feet of water in the arm, will be equal to the pressure of sixteen feet of water in an upright tube. This, added to the pressure from the upright tube, will make the whole equal to a height of

thirty-two feet; and the pressure from which the side EG is now relieved at the aperture, will be equal to that of a column whose height is thirty-two feet, and whose transverse section is equal to the area of the aperture. This is the force with which the arm will be pressed backward in the direction A*a*.

But there is now a very considerable resistance opposed to this motion of the arm. This resistance, as observed above, is as the quantity of water that is continually moving along the arm, and the velocity that is given to it in the direction H*h*. The quantity of water passing along the arm is regulated by the velocity with which it issues from the aperture, and the transverse section of the contracted vein. The velocity is in this case about 45.25 feet per second; and the contracted vein is to the aperture from whence it issues nearly as 64 to 100. If we denominate the area of the aperture by unity, then the contracted vein will be the decimal .64; this multiplied by 45.25, the length of the spouting column per second, gives 28.96; this quantity of water is uniformly accelerated in the direction H*nh*, at the rate of thirty-two feet per second, in the same manner as if it were subjected to the action of gravity; and, reaction being equal to action, the resistance will be equal to the weight of this quantity of water pressing upon the side AF. But the whole resistance is the difference of the pressures upon the two sides AF and EG; and the tendency of the water to fall back from the side AF being equal to the pressure upon the side EG, the above quantity must be doubled: This gives 57.92 for the whole resistance. But, again, this resistance presses equally along the whole length of the arm from G to H; the centre of resistance, therefore, lies but half-way betwixt G and H, whilst the centre of the impelling power lies upon the point *n*, acting as it were

with a lever of double the power that the resistance does: Consequently, the above quantity 57.92 must be divided by 2, which again reduces the resistance at H, to 28.96. But the impelling power is equal to thirty-two; it is therefore capable of communicating a still greater velocity to the arm in the direction H*nh*.

Let the velocity of the point *n* be forty feet per second: The centrifugal force increasing as the square of the velocity, the pressure of the water in the arm, from this force at the point *n*, will now be equal to the weight of a column of water twenty-five feet in height; this added to the pressure from the upright tube gives the impelling power, or the relief from pressure, at the point *n*, equal to forty-one. But the resistance will be found to have increased still more than the impelling power; so that now they nearly balance one another. The velocity with which the water will now issue from the aperture, supposing the jet to be complete, will be 51.22 per second; this multiplied by .64, the transverse section of the contracted vein, gives 32.78 for the quantity of water that moves along the arm in a second of time. This quantity of water is accelerated in the direction H*nh* at the rate of forty feet per second; being one-fourth more than the acceleration by the power of gravity: we must therefore increase the above quantity 32.78 in this proportion; which gives 40.976 for the resistance, coinciding very nearly with the impelling power, 41. Hence we see that the resistance, from the water moving along the arm, increases much faster than the impelling power; and that the velocity of the arm is a maximum, the height of the upright tube being sixteen, when the point *n* moves with the velocity of forty feet per second; or generally, when the point *n* moves with a velocity one-fourth greater than that which a body would ac-

quire, by gravity, in falling through the height of the upright tube.

But the above merely determines the greatest velocity the arm can acquire when the machine is unloaded: We must not in this case even suppose any kind of friction. It is evident that it must move slower than this when it is employed for any practical purpose, or to produce any mechanical effect. Its power to produce such effect must be as the difference betwixt the impelling power, and resistance it experiences within itself from the water in the arm, multiplied by the velocity of the arm; and its efficiency is the greatest when the effect produced bears the greatest proportion to the power expended. We shall here make a few hypothetical trials of the power and efficiency of this machine, when the arm is moving with different velocities.

I.

Let the velocity of the arm, at the aperture, in feet per second, be - - -	12.00
The impelling force will then be - -	18.25
Resistance from the water in the arm, -	8.20
Load it is capable of raising with the velocity of the point *n*, - - - - -	10.05
Effect, being the load multiplied by the velocity of the arm at the point *n*, - -	120.60
Expenditure of power, or the quantity of water multiplied by the height through which it has descended, viz. 16 feet in one second,	349.76

Ratio of effect to expenditure, 1 : 2.9.

II.

Velocity of the arm, at the aperture, in feet per second, - - - -	14.00

Impelling force, - - -	19.06
Resistance from the water in the arm, -	9.78
Load which it is capable of raising with the velocity of the point *n*, - - -	9.28
Effect, - - - - -	129.92
Expenditure of power, - - -	357.60

Ratio of effect to expenditure, 1 : 2.76.

III.

Velocity of the arm, at the aperture, in feet per second, - - - - - -	16.00
Impelling force, - - - -	20.00
Resistance from the water in the arm, -	11.14
Load which it is capable of raising with the velocity of the point *n*, - - -	8.55
Effect, - - - - -	136.86
Expenditure of power, - - -	366.27

Ratio of effect to expenditure, 1 : 2.67; being then a maximum. Velocity with which the water moves backward, in this case, on leaving the machine, 19.77 feet per second. Power of ditto, 33.

IV.

Velocity of the arm, at the aperture, in feet per second, - - - - - -	20.00
Impelling force, - - - -	22.25
Resistance from the water in the arm, -	15.09
Load which it is capable of raising with the velocity of the point *n*, - - -	7.16
Effect, - - - - -	143.20
Expenditure of power, - - -	386.33

Ratio of effect to expenditure, 1 : 2,7.

V.

Velocity of the arm, at the aperture, in feet per second, - - - - -	22.00
Impelling force, - - - -	23.55
Resistance from the water in the arm, -	17.06
Load which it is capable of raising with the velocity of the point *n*, - - -	6.49
Effect, - - - - -	142.78
Expenditure of power, - - -	397.60

Ratio of effect to expenditure, 1 : 2.78.

VI.

Velocity of the arm, at the aperture, in feet per second, - - - - -	24.00
Impelling force, - - - -	25.00
Resistance from the water in the arm, -	19.20
Load which it is capable of raising with the velocity of the point *n*. - - -	5.80
Effect, - - - - -	139.20
Expenditure of power, - - -	409.60

Ratio of effect to expenditure, 1 : 2.94.

VII.

Velocity of the arm, at the aperture, in feet per second, - - - - -	32.00
Impelling force, - - - -	32.00
Resistance from the water in the arm, -	28.99
Load which it is capable of raising with the velocity of the point *n*, - - -	3.01
Effect, - - - - -	96.32
Expenditure of power, - - -	463.84

Ratio of effect to expenditure, 1 : 4.87.

From these trials it appears, that this machine is working to the greatest advantage when that part of

the arm, in which the aperture is situated, is moving with one-half the velocity which a body would acquire, by gravity, in falling through the height of the upright tube. As in the undershot wheel, however, so it is here: a considerable latitude may be used with respect to the velocity, without greatly diminishing the effect. In a machine of the above proportions, any velocity, from twelve to twenty-four feet per second, may be employed without diminishing the effect much more than one-tenth. It is the load that regulates the velocity, by retarding the motion; and it appears, likewise, from the above trials, that to produce the greatest effect, the load should be little more than one-half of that with which it can just move from a state of rest. It also appears from these trials, that, at the maximum, the effect is to the power expended as 10. to 26.7. This gives the effect considerably greater than has been determined by some mathematicians, whilst it is much less than what has been assigned to it by others. Mr Playfair, in his "Outlines of Natural Philosophy," makes the effect of this machine equal to the momentum of the water expended; so that it should, if there was no power lost by friction, raise up the whole of the water to the height from which it fell, and in the same time. "It is therefore," says he "the most advantageous application of water that is known." But he observes, that in practice, the result will fall much short of this theory, on account of the greatness of the friction. The friction, no doubt, is very considerable, from the great weight of water that must be continually in the upright tube and arms at a time. This must always be much greater than the load upon an overshot wheel of the same height, and where the expenditure of water in a given time is the same; and the diminution of effect from

this cause, will be the greater, as the arms are shorter, the weight of water in the machine being the same; for it must then make a greater number of revolutions in a given time. But, on the other hand, the friction of a machine moving horizontally on a vertical pivot, is always less than that of one moving vertically on an horizontal axle. In the latter case, the axle requires to be thicker, and the whole weight turns upon its circumference; but on a vertical pivot the weight turns as much on the centre as the circumference, which greatly lessens the friction. Besides, there is one circumstance attending this machine, which, in practice, makes the effect greater than it is by theory; or rather, there is a favourable circumstance attending this machine which we have omitted in the above theory; for theory, if it is perfect, must always agree with practice. It has been found by experiment, * that in making an aperture near the bottom of a vessel filled with water, the relief from pressure on that side is greater than what is due to the mere area of the aperture. This would appear to be owing to the pressure of the water, on the parts immediately around the aperture, not being so complete as if the aperture were shut. This, when applied to the machine in question, in the first place, increases the impelling power; and, secondly, by reducing the velocity of the water from the aperture, it diminishes the quantity continually moving along the arm; and consequently it lessens the internal resistance of the machine. This should, in a great measure, counterbalance the friction: yet we do not find, in practice, that this machine can return near the whole of the water expended. The effect, in one instance, has been found † to be to the

* By the author. † Ibid.

power expended, nearly as 10 to 31.7: the theory we have advanced makes it as 10 to 26.7. In a number of trials, it was found that the effect was a maximum, when the load was just one-half of that with which it could just move from a state of rest; and the velocity of the arms, at the aperture, one-half of that which a body would acquire, by gravity, in falling through the height of the upright tube: in both these respects, agreeing very nearly with our theory. The efficiency of this machine, in practice, will be found at least equal to that of an undershot wheel—that is to say, a wheel acting by impulsion, or on pure undershot principles—but must be greatly inferior to an overshot wheel, or an undershot, acting, as we have observed these wheels are usually made to do, upon the overshot principle. Mr Playfair differs, likewise, greatly from our theory, in regard to the velocity with which he supposes this machine should move. "When the machine is working to the greatest advantage," says he "the velocity with which the water issues, is equal to that with which it is carried horizontally in an opposite direction; so that, on coming out, it falls perpendicularly down." If this theory of Mr Playfair's were correct, in the above case, when the effect was a maximum, the velocity should have been nearly 35.8 feet per second, instead of 16, as we have found it both by theory and experiment.

The above proportions, which we have assigned to this machine, must no doubt appear strange, or even ridiculous: arms, thirty-two feet in length, would be quite cumbrous in use, and almost impracticable in construction. But we by no means propose such proportions in practice: we merely adopted these numbers, sixteen feet for the upright tube, and thirty-two for the length of the arms, as being the most conven-

ient for elucidating the subject, coinciding with the effects produced by gravity, in a second of time. But the effect will be the same whether the arms are short or long: If the quantity of water in the arms is less, by the arms being shortened, the centrifugal force of any given quantity of water will be proportionally increased so long as the velocity of their extremities is the same; for it is a well known property of circular motion, that the angular velocity being the same, the centrifugal force is increased in proportion as the radius of the circle is diminished; consequently, *cæteris paribus*, the whole centrifugal force of the water in an arm of three feet in length will be same as in one of thirty-two feet. And, again, the internal resistance of the water in the arm will be the same; for the same quantity of water passes along it, and has communicated to it the same angular velocity in the same time.

Upon the whole, we see that water will produce very different effects, as it is applied to one or other of these different kinds of machines; and that where it is required that the power employed should produce the greatest effect, the overshot wheel, or the wheel acting on the overshot principle, is always to be preferred. We see, too, that in each of them the effect bears always a certain proportion to the height through which the water descends; varying always directly as that height. This is well known to every, even the rudest, practical engineer, who, when he once knows the effect that a machine will produce with a given quantity of water descending from a given height, can at once, by this simple rule, determine that, with a double or quadruple height, and a machine of the same kind, suited to that height, it will, *cæteris paribus*, produce a double or quadruple effect. This corresponds perfectly with that measure of power which we adopted in re-

gard to solid bodies, viz. the square of the velocity, which is always directly as the perpendicular height through which a body falls in free space.

WINDMILLS.

THE mode of application of air to the moving of machinery differs very considerably from that of water. It is commonly applied to an oblique surface, which it causes to move round vertically at right angles to the direction of the air itself. This certainly does not at first sight appear to be the most natural way of applying any moving power. Accordingly, many attempts have been made with horizontal moving machines to obtain a more direct motion; and it is said that these machines are actually in use in some parts of the world; but it has never been found practicable to give them any thing like the power or efficiency of the vertical kind. Smeaton is perhaps not far from the truth, when he reckons the vertical windmill to be eight or ten times more powerful than the horizontal.

Smeaton, in accounting for the great superiority of the vertical mill over the horizontal, attempts to demonstrate, that there is no power lost by the wind acting obliquely on the sails—" *That all sails, however situated, that intercept the same section of the wind, and having the same relative velocity in regard to the wind, when reduced into the same direction, have equal powers to produce mechanical effects.*" His demonstration is as follows:

" In Fig. 3.*, let AB be the section of a plane, upon which let the wind blow in the direction CD, with such a velocity as to describe a given space BE, in a given

* Experimental Enquiry, page 64.

time (suppose one second); and let AB be moved parallel to itself, in the direction CD. Now, if the plane AB moves with the same velocity as the wind—that is, if the point B moves through the space BE in the same time that a particle of air would move through the same space—it is plain, that, in this case, there can be no pressure or impulse of the wind upon the plane; but if the plane moves slower than the wind, in the same direction, so that the point B may move to F, while a particle of air, setting out from B at the same instant, would move to E, then BF will express the velocity of the plane; and the relative velocity of the wind and plane will be expressed by the line FE. Let the ratio of FE to BE be given (suppose $2:3$); let the line AB represent the impulse of the wind upon the plane AB, when acting with its whole velocity BE; but, when acting with its relative velocity FE, let its impulse be denoted by some aliquot part of AB, as, for instance, four-ninths AB; then will four-ninths of the parallelogram AF represent the mechanical power of the plane—that is, $\frac{4}{9}AB \times \frac{1}{3}BE$.

"*2dly*, Let IN be the section of a plane, inclined in such a manner, that the base IK of the rectangle triangle IKN may be equal to AB; and the perpendicular NK=BE; let the plane IN be struck by the wind in the direction LM, perpendicular to IK: then, according to the known rules of oblique forces, the impulse of the wind upon the plane IN tending to move it according to the direction LM, or NK, will be denoted by the base IK; and that part of the impulse, tending to move it according to the direction IK, will be expressed by the perpendicular NK. Let the plane IN be moveable in the direction of IK only; that is, the point I in the direction of IK, and the point N in the direction NQ, parallel thereto. Now, it is

evident, that if the point I moves through the line IK, while a particle of air, setting forwards at the same time from the point N, moves through the line NK, they will both arrive at the point K at the same time; and, consequently, in this case also, there can be no pressure or impulse of the particle of the air upon the plane IN. Now, let IO be to IK as BF to BE; and let the plane IN move at such a rate, that the point I may arrive at O, and acquire the position OQ, in the same time that a particle of wind would move through the space NK: as OQ is parallel to IN, (by the properties of similar triangles), it will cut NK in the point P, in such a manner, that NP=BF, and PK=FE: Hence it appears, that the plane IN, by acquiring the position OQ, withdraws itself from the action of the wind, by the same space NP, that the plane AB does by acquiring the position FG; and, consequently, from the equality of PK to FE, the relative impulse of the wind PK, upon the plane OQ, will be equal to the relative impulse of the wind FE, upon the plane FG: and since the impulse of the wind upon AB, with the relative velocity FE, in the direction BE, is represented by four-ninths AB, the relative impulse of the wind upon the plane IN, in the direction NK, will in like manner be represented by four-ninths IK; and the impulse of the wind upon the plane IN, with the relative velocity PK, in the direction IK, will be represented by four-ninths NK; and, consequently, the mechanical power of the plane IN, in the direction IK, will be four-ninths the parallelogram IQ: that is, $\frac{1}{3}IK \times \frac{4}{9}NK$: that is, from the equality of IK=AB and NK=BE, we shall have $\frac{4}{9}IQ = \frac{1}{3}AB \times \frac{4}{9}BE = \frac{4}{9}AB \times \frac{1}{3}BE = \frac{4}{27}$ of the area of the parallelogram AF. Hence we deduce this

"GENERAL PROPOSITION,

"*That all planes, however situated, that intercept the same section of the wind, and having the same relative velocity, in regard to the wind, when reduced into the same direction, have equal powers to produce mechanical effects.*

"*For what is lost by the obliquity of the impulse, is gained by the velocity of the motion.*"

All this would do very well if Mr Smeaton had first shown, that the wind strikes with the same force upon the oblique plane IN when at rest, that it does on the plane AB; but this he has most unaccountably overlooked. In the above case, it is not the obliquity of impulse that is counterbalanced by the greater velocity; it is the smallness of the proportional tendency of the force with which it is struck to move the plane in the direction IK, that is compensated by the greater velocity in that direction. But the absolute force of the wind upon IN never can be so great as that upon AB, though it may be brought infinitely near to it, by infinitely reducing NK.

From this, then, we should be led to conclude, that the more we reduce NK the greater should be the effect. If there was no friction, and the edge of the sail or vane was infinitely thin, the effect would certainly be the greatest when NK is infinitely reduced; but this in practice very soon has limits, owing to the friction, and the resistance of the air to the sides of the vanes. Friction is nearly a constant quantity; that is to say, constantly the same to a body passing over the same space: Therefore, if NK is reduced, the friction, continuing the same, will bear a greater proportion to it; and the velocity in the direction IK being proportionally increased, the resistance of the air to the edge of

the wind: that is to say, when the plane IN assumes the position OQ, in the time that a particle of air moves through the space NK; IO being one-third of IK. This is confirmed by Smeaton's experiments, in which it appears that the velocity of the sails at a maximum, was nearly one-third of that when the machine was unloaded, when it might be supposed to be moving with nearly the whole velocity of the wind: that is to say, when the point I would move through the space IK, in the time that a particle of air would move through the space NK.* This would make the efficiency of a vane of the horizontal mill, compared with one of the vertical, as one-half to eight twenty-sevenths.

But, on the other hand, this is much more than counterbalanced by another circumstance necessarily attending the horizontal vane, viz. that there is only an infinitely small part of it whose velocity can be adjusted so as to produce a maximum effect. For instance, if we suppose the vane to extend from the axis, or centre of motion, and that, at half way from the axis to the extremity, it moves with half the velocity of the wind; that part, or the middle of the vane, will then be moving so as to produce a maximum effect: but then the extremity will be moving with the whole

* When we say, that the velocity of the sails should thus bear a certain ratio to that of the wind, this may seem to imply, that the velocity of the sails should always vary with that of the wind; which would be very unsuitable in practice. Mills commonly being required to move at some certain rate, according to the purpose to which they may be applied. The above can only be meant for some moderate rate of wind, which requires the most advantageous position of the sails; for in higher winds the greatest effect is never required. But it is not enough that the sails are put in the most advantageous position: it is evident, that unless the load is properly adjusted to the power, or the power to the load, the sails will not move with that velocity which will produce the greatest effect.

velocity of the wind; and, of course, the wind can have no effect upon that part. Again, next the axis the wind will be striking with its whole velocity; but there the velocity of the vane is nothing; and, consequently, that part likewise can be producing no effect. So that from the middle of the vane, both outward and inward, to both extremities, the power decreases to nothing. This reduces the effect about one-half. The loss of power, however, from this cause, is not so great where the nearest side of the vane is at some distance from the axis; but still it must be very considerable, and seems without remedy. But in vertical mills, by accommodating the inclination of the different parts of the sails to their respective velocities, there is comparatively little power lost in this way. If the velocity in the direction IK is greater towards the extremity of the sails, we have only to reduce NK; and though the force, with which the plane is pressed in the direction IK, is less than at the middle of the vane, its velocity is greater; so that its efficiency will be nearly the same. Again, as the velocity of the parts towards the axis is less, we increase NK; so that though we lose in velocity in these parts, we gain in power. Hence, we can bring all the several parts of the vane to act with nearly the same efficiency; which it is not possible to do in the case of an horizontal vane. Yet even in vertical mills the parts do not all operate with quite the same effect. There is only one part, rather beyond the middle of the vane, which we shall term the centre of action, to which the most advantageous inclination can be given; and this can be determined only by experiment, being much affected, as above observed, by friction, and the resistance of the air to the sides of the vanes; and the friction being altogether, and the resistance to the sides of the vanes almost, beyond the

reach of calculation. The inclination and velocity of the centre of action being determined, the several inclinations of the other parts come more within the reach of calculation. We have no choice as to the velocities of those parts, which must necessarily be to that of the centre of action as their respective distances from the axis; we can only, as to them, adapt NK to IK, so as to produce the greatest effect, with the given velocity, in the direction IK. That is to say, we must so proportion NK to IK, that NK may be equal to one-third of the space through which a particle of air moves in the time that the point I moves through the space IK: the air will then strike the plane with two-thirds of its velocity. From this it is evident that the proportion of NK to IK must always be made to decrease towards the extremity of the vanes.

A separate calculation is thus required for each of the several parts of the vane; and, even after this is obtained, regard is to be had in adjusting them to one another, that, taken collectively, they may form a somewhat concave surface towards the wind; though this will require a twisting that will be to the disadvantage of some of the parts taken separately. It is not enough that the parts taken separately have each the most advantageous inclination, unless their form is likewise the best when taken as a whole. If, when put together, they make a convex surface, it is evident that the wind, which moves off alike in every direction, according to the inclination of the surface which it strikes, will lose the advantage of the best inclination in one direction by a wrong inclination in another. The inclinations of the parts should be so adjusted to one another as to make the general surface presented to the wind somewhat concave.

From all this, we see the absurdity of those rules

laid down by certain mathematicians, who have determined that the inclination of every part of a vane, and under all circumstances, should be the same, viz. 35 deg. to the plane of its motion; and which Smeaton found to be the most disadvantageous of all the angles he tried. We see, too, that to obtain with rigorous exactness the best form of these vanes forms a very intricate question; but it fortunately happens here, as in water-mills, that a small deviation from the very best form does not produce any considerable diminution of efficiency.

There is another circumstance which we have not yet adverted to, that, in practice, gives the vertical mill a great advantage over the horizontal; viz. its superior steadiness, in consequence of the greater velocity of the vanes. One of the greatest objections to windmills in general, is their want of steadiness: whatever, therefore, can obviate this defect, must be of the greatest consequence. The velocity of the vanes of the vertical mill, when acting to the greatest advantage, may be reckoned three times that of the horizontal: its steadiness, or its tendency to persevere during any relaxation of the pressure of the wind, or its power to overcome any temporary increase of resistance, being as the square of the velocity, will therefore be nine times greater than that of the other. But the greatest of all the objections to the horizontal mill is, the difficulty of bringing back the sails or vanes against the wind; and the circumstance, that only one vane can be made to act with any sort of efficiency at a time; whilst, in the vertical kind, four or five vanes go on continually operating with their full power. When to these we add the other disadvantages already mentioned, we must certainly agree with Smeaton, when he estimates the power of the vertical mill at

eight or ten times that of the horizontal. How erroneous, then, must be the ideas of some of our scientific men, who are still urging horizontal mills upon the serious attention of the world.

It may be expected that we should here make some observations on the absolute effect of windmills.

There is a very considerable difference betwixt the impulsion of an uncompressed or unconfined column of fluid acting against a resisting surface, such as a column of water upon the floatboard of an undershot wheel, where, after the impulse, the water has full room to escape on every side, and that of a column of compressed fluid, such as a column of air intercepted by the sail of a windmill. In the former case, the fluid strikes with a force according to the angle which the resisting surface actually presents to it. If the surface stands at right angles to the direction of the water, the water strikes it at right angles; or if the surface stands at any given angle of inclination, the water will strike it with a force according to the sine of that angle. But it is not so with the air. After the particles of the air have struck the surface, they are prevented by the surrounding fluid from flying off immediately from before it—they are arrested in a certain degree before it—and thereby prevent the succeeding particles from striking it perpendicularly, though the surface may be standing at right angles to the direction of their motion. This is found to have the same effect in deflecting the fluid as if the resisting surface itself had a certain degree of roundness. The particles of air at the middle of the plane being the longest in making their escape from before it, gives the resisting surface virtually a rounded form. The force of impulsion is thereby very much reduced; so that it is found by experiment to be less than it would be if

the air actually struck the surface at right angles, in the ratio of 3.91 to 5.38. When the resisting surface is inclined to the direction of the fluid, the force of impulsion is less affected by this cause: The fluid, after having struck the surface, being in that case less arrested upon it, in the ratio of the sine of incidence to radius. The force of impulsion is to be calculated first as if the angle of the resisting surface was the real angle, and then a deduction made from it in proportion to the resistance thus found, in a ratio as the sine of incidence to radius; the deduction at 90 deg. being as 1.47 to 5.38.*

Mathematicians have supposed (though erroneously,) that an undershot water-wheel is acted upon on the principle of a surface moving continually onward in the direction of the water, which overtakes it with a relative velocity; and, accordingly, they have supposed, that both the quantity and force of the water striking it are as this relative velocity, and consequently have determined the effect of the water to be eight twenty-sevenths of the power expended. This is really the principle on which the wind acts on the sails of a vertical windmill; only with this difference, as observed above, that a column of air, on account of its being confined by the surrounding fluid, acts with less force than an unconfined column of water of equal momentum, in the ratio of 3.91 to 5.38. The efficiency of a vertical sail is likewise subject to a farther small diminution on account of its obliquity; after both these deductions are made, the total effect, instead of eight twenty-sevenths, will be somewhat less than one-fifth of the power expended. That is to say, the effect produced by each sail in a given time will be somewhat

* For this, see more fully the Resistance of Fluids.

less than one-fifth of the momentum of the column of air intercepted by it: the transverse section of the column being computed by the surface which the sail presents to the wind, measured in a direction at right angles to that of the wind; and the length of the colum by its velocity, or the space it passes over in that time. This is the greatest effect it can produce according to theory, without any allowance for either friction or the resistance of the air to the sides of the vanes; but, in practice, a considerable allowance must be made for both of these.

SEAMANSHIP.

By far the most extensive use to which wind is applied is the communicating motion to ships. The theory of the application of it to this purpose is much more complicated than to mills. The number and variety of form of the sails are greater; and the surface of these sails not being planes, it is difficult to determine the position of their surfaces. In the case of windmills the resistance is nearly constant or uniform, or at least it is supposed to be so; but here the resistance increases with the velocity of the ship: hence the subject involves likewise the Resistance of Fluids. This resistance of the water to the hull of the ship depends much on its form and dimensions, which are hardly the same in any two ships. When to all this we add the difference betwixt the required course and the direction of the wind, and the lee-way hence arising, there is little room left for the philosopher or the mathematician entertaining sanguine ideas of being able to form rules, that can be of much use to the seaman to direct him in his practice.

Yet a knowledge of the theory of the percussion and

resistance of fluids in general, must be highly useful both to the shipbuilder and the seaman: It must be desirable for them to know the reason of the several forms and positions of the different parts of the ship; and the way in which changes in these forms and positions produce their different effects. This both facilitates the acquisition of knowledge in the course of their practice, and causes that knowledge to make a more lasting impression on the mind.

THE RESISTANCE OF FLUIDS.

By the resistance of fluids, we understand that resistance which a body experiences in moving through a fluid. This forms a still more intricate subject than the percussion of a fluid against a body not immersed in it. In the latter case, we generally have a pretty correct knowledge both of the quantity of the acting fluid, and the direction of its action. But it is very different in the resistance of fluids. When a body is moving through a fluid, it displaces not merely the particles that lie directly in the path of its motion: these must likewise displace, in a certain degree, those that lie contiguous to them; and these latter, again, those that lie beyond them; so that the motion becomes ultimately very diffuse.

The opinions of philosophers on this subject are still very unsettled, generally differing from one another; many of them inconsistent even with themselves, and almost all of them disagreeing with experiment.

Sir Isaac Newton, in his "Principles of Natural Philosophy," B. II., Prop. 35, puts an imaginary case of a discontinued fluid, or a fluid the particles of which are freely disposed at equal distances from one another. He first puts the case of these particles being perfectly

elastic; and he determines the resistance which a cylinder of the same density experiences in moving through such a fluid, to be equal to the force by which its whole motion may be either generated or destroyed in the time that it describes one-half its length. He puts another case, in which he supposes other circumstances to be the same, but the particles to be non-elastic, or not to be reflected. In this case, the resistance is but half as great as in the former: this resistance is, in other words, equal to the weight of a column of the fluid whose transverse section is equal to that of the cylinder, and whose length is double the height due to the velocity. All this is in perfect conformity with our preceding theory. But, again, Book II., Prop. 37, he determines the resistance of a compressed, infinite, and non-elastic fluid to be but one-fourth part of this latter resistance, or the eighth part of the former. Now, the resistance of the air with regard to slow motions, such as ten or twelve feet per second, certainly comes very near to this last case; for although the air may be perfectly elastic, yet, from its continuity, that elasticity has no power to exert itself, having no room to rebound from the moving body. It may therefore be considered, with regard to its resistance, as non-elastic; and from its instantly filling up the space behind bodies moving with those slow motions, it may be regarded as perfectly compressed and continuous. But experiment proves that the resistance of the air, even in these slow motions, to a thin board, is nearly three times greater than Newton has thus determined for a compressed, infinite, and non-elastic fluid; or very nearly equal to three-fourths of what he has assigned for a fluid unelastic and discontinued; or it is nearly equal to three-fourths of the force of a column equal to that displaced by the moving body, acting

perpendicularly against a fixed plane, where it has full room to escape on every side after collision.

Some authors have objected to those who complain, that they have not found the theory of Newton to agree with experiment, that they do not make allowance for the air not being sufficiently compressed to fill up instantly the space behind the body. But this plea will not hold good as to these very slow motions; and with regard to very high velocities, where the compression is really not sufficient instantly to fill up the vacuum that is always forming behind, it has been found that the disagreement betwixt experiment and the theory of Newton is still much greater than we have observed above. The Newtonian theory of the resistance of fluids is certainly a complete failure, and not to be supported. The name of Newton has solid ground enough on which to stand, without being bolstered up on that which is hollow and unsound.

All fluids, like other bodies, are inert; all fluids are more or less continuous and compressed; all fluids are in a certain degree elastic, though their elastic power, in as far as respects their resistance, may be counteracted by their compression and continuity. A body moving through a fluid must be affected by all these circumstances, which together constitute the resistance of the fluid; and before we can understand the true nature of that resistance, we must analyse it, or separate it into its component parts. There is no other way by which we can attain to accuracy in this, or indeed in any other subject.

When the solid body which moves through the fluid has a plain front, and moves forward in a right line perpendicular to its plane, the particles struck by the front are prevented by the continuity of the fluid from flying off immediately from before it, and are in some

degree carried forward by it. This has the same effect upon the succeeding particles as if the front of the body were convex: It causes them to move off to the sides with a somewhat circular motion, instead of striking the front perpendicularly, and consequently very much diminishes their effect. But, on the other hand, the resistance of the particles, that lie immediately in the path of the body, does not constitute the whole of the resistance. In pressing these aside, they press others that lie next to them; these, again, others that lie beyond them, and so forth; and the motion communicated to all these must augment the sum of resistance. It appears by experiment that the augmentation of resistance in this way in a considerable degree counterbalances the diminution of it, by the obliquity of the impulse of those particles that lie directly in the path of the body's motion. As we formerly observed, the resistance to a thin board moving through the air, and standing at right angles to the path of its motion, is nearly equal to three-fourths of the force of a column equal to that displaced by the board, and striking perpendicularly against a fixed plane.

It is not with bodies of every form, however, that the resistance is so great, even though the form of their front should be the same. If we make the same experiment upon the end of a long box of similar dimensions, and moving in the direction of its length, we find the resistance considerably less; and the reason of the difference is this: In the case of a thin board, the particles of air next the edge of it fall immediately into the vacuum that is always forming behind it, thereby making room for the particles, after striking the front of it, to recede more immediately from before, and to allow the succeeding particles to strike more perpendicularly, and consequently with greater effect. But

in the case of a long box, the vacuum that is forming behind it is at a much greater distance from the striking surface, or the fore end of the box; the motion of the surrounding fluid towards the vacuum at that distance is more diffuse and slower; the recession of the particles from before is more interrupted; the succeeding particles are deflected with greater obliquity; they consequently act with less force against the end of the box than if it were a thin board. Even the form of the hind end has a considerable effect in this way.

Here, again, experiment shows the erroneousness of the ideas of Newton on this subject. In Book II., Lemma 4., he assumes the resistance in both these cases to be the same: "If a cylinder," says he, "move uniformly forward in the direction of its length, the resistance made thereto is not at all changed by augmenting or diminishing that length, and is therefore the same with the resistance of a circle, described with the same diameter, and moving forward with the same velocity in the direction of a right line perpendicular to its plane;

"For the sides are not at all opposed to the motion; and a cylinder becomes a circle when its length is diminished *in infinitum*."

Now, experiment shows, that the resistance to a thin board of a foot square is to the resistance to the end of a box, of the form and dimensions of a cube of 1 foot, as 391 to 322; and to a longer box, but with its end of the same dimensions, the difference is still greater.

Philosophers, in their opinions on these subjects, are not only at variance with experiments and with one another; but we sometimes find the strangest incongruities in the works of the same author. What, for instance, can we think of the following passage of

Hutton's, when we compare it with his own statement of some experiments, which follows, in the course of a few pages after, in the same work? "In the discontinued fluid, first described," says he, "the obliquity of the foremost surface of the moving body would diminish the resistance; but the same thing does not hold true in compressed fluids, at least not in any considerable degree; for the chief resistance in compressed fluids arises from the greater or the less facility with which the fluid, impelled by the fore part of the body, can circulate towards the hinder part; and this being little, if at all, affected by the form of the moving body, whether it be cylindrical, conical, or spherical, it follows, that while the transverse section of the body is the same, and consequently the quantity of impelled fluid also, the change of figure in the body will scarcely affect the quantity of its resistance." * Yet, notwithstanding this, only four pages after, he gives us the following as the experimented resistances of the air, which is certainly a compressed fluid, to bodies of these different forms, having their tranverse sections quite the same, and all moving with the same velocity of four feet per second; viz. a cone, with the vertex foremost, .048 oz.; ditto, with the base foremost, .109 oz.; a cylinder, .09 oz.; a whole globe, .047 oz. Such glaring inconsistency as this, shows a perfect trifling with the subject.

The continuity of a fluid always lessens its resistance, by retarding the recession of the particles from the fore end of the moving body, and thereby giving it virtually a rounded form. The compression of a fluid never lessons the resistance; but, on the contrary, often

* Mathematical Dictionary—Article, RESISTANCE OF FLUIDS.

forms a distinct part of it. Where the compression is infinite, or even so great as instantly to fill up the vacuum behind the moving body, it then presses alike in every direction, and consequently produces no effect. The resistance of a fluid, where the compression falls short of this, may be divided into two parts: First, that which arises from its inertia, of which we have hitherto been treating, and which is regulated entirely by the real or virtual form of the fore end of the moving body. By the virtual form, we mean that disposition which the body posseses of deflecting the particles of the fluid from the path of its motion; and this, we have seen, depends not only on the actual form of the fore end, but likewise on whatever causes it to carry forward the fluid before it; and this, again, depends partly on the length of the body, and the form of its hind end, by affecting the recession of the particles from before, which has the same effect in deflecting the succeeding particles as if the fore end of the body itself possessed a certain degree of roundness.

The other part of the resistance arises from the compression of the fluid; and this is quite independent of its inertia. If a body is immersed in a fluid without motion, it will be pressed by the fluid equally on every side; and the pressure will be as the density of the fluid, and the depth to which the body is immersed in it. If a certain degree of velocity is communicated to the body, but the velocity not so great but that the horizontal motion of the fluid following after it, arising from its compression, shall instantly fill up the space left by the moving body, the weight of the superincumbent fluid will still cause that part of the fluid, which has filled up the space behind, to press upon the hind end of the body with nearly, but not quite, the same force

as if it were at rest.* But if the compression of the fluid is not sufficient to fill up the space behind the body, but leaves either a perfect or a partial vacuum, the fore end will then be pressed with a force which is not counterbalanced behind. If the body is moving at the surface of the fluid, as a ship on the water, however slow the motion of the vessel may be, the fluid will not rise so high on the hind end as on the fore. This pressure upon the fore end of the vessel, which is not counterbalanced behind, may be compared to that against a lock by which the water is dammed up in a canal to a higher level. The pressure of the water against the lock in the longitudinal direction of the canal, does not at all depend on the form of the lock, whether that be plain or convex, but is directly as the width of the lock measured straight across the canal. The pressure against any part of the lock is as the depth of that part from the higher surface of the water; and the total pressure against the lock is as its width, and the square of the height of the surface of the water in the higher level above the lower; but without any regard to the form of the lock:† so the resistance to a ship in the water, in as far as it arises from compression, is no way affected by the form of its fore end.

In the case of a body moving through the air, at least in the lower part of the atmosphere, the velocity must be very great before a vacuum can be formed behind it. Accordingly, in this case, it has been alleged that the resistance increases at a very rapid rate; or

* The vacuum is filled up, not only from the sides and from below, but likewise from above; this must detract from the pressure of the superincumbent fluid.

† The form of these locks is commonly convex towards the higher level; but they are made thus with a view to strengthen the lock—not to lessen the pressure against it.

even almost instantly, in those high velocities, when a vacuum is supposed to take place. But we cannot see how the resistance should increase so very rapidly, as the great elasticity of the air must continue to fill up the space behind with air of a decreasing density long after a disposition to a vacuum begins.

But the resistance from compression, though not at all affected by the form of the fore end, is very much affected by that of the hind end of the moving body. If that body is of the form of a parallelopiped, and moving with a velocity greater than the compression at that depth can produce in the fluid, there must be a vacuum formed behind it. But if, instead of the body being a parallelopiped, its hind end is of a converging form, or consists of a long tapering tail as it moves along, the fluid will have more time to close in upon it from the sides; and, in proportion as the length of the tapering part exceeds half the width at the broadest part, so much faster may the body move without leaving a vacuum in it. But if the body is moving at the surface of the fluid, as a ship on the water, the vacuum behind can never be perfectly filled up, however slow the body may be moving, the compression at the very surface being nothing. Hence, experience has led to that tapering form universally given to these vessels; yet however much the resistance from compression may by this means be reduced, it can never be altogether done away, as the surface of the water will be always somewhat lower behind than before. Unless the tapering part of the moving body is longer than half the width of the broadest part, it can have no effect at all in lessening the resistance; for it can receive no impulse from the lateral pressure of the the fluid, as the fluid following from behind will fill up

the vacuum before that from the sides can reach the middle.

From the obliquity of impulse which arises from the fluid being partially carried forward by the front of the moving body, and the consequent diminution of that part of the resistance which arises from the inertia of the fluid, we have an easy and complete solution of the difficulty that has always been found in determining the resistance to a plain board set at different angles of inclination to the path of its motion; and which forms one of the most important problems in the resistance of fluids.

It has frequently been assumed by mathematicians, that the resistance to an inclined plane must be as the square of the sine of incidence; for the force of the fluid, the quantity being the same, must be as the sine; but the breadth of the column, or the quantity of the fluid intercepted, must be likewise as the sine; therefore the whole resistance should be as the square of the sine. But this has not been found to agree with experiment. If the apparent were the real or virtual angle of incidence, this certainly would be the law of the variation of resistance. But this is not the case. We see that when the board is set at right angles to the path of its motion, the fluid does not strike it perpendicularly; so likewise when the board is inclined at a certain angle, the obliquity of the angle at which the fluid strikes it, is considerably increased by the fluid that is carried forward by it. The degree in which the fluid is carried forward by the inclined board, and the consequent increase of the obliquity of the impulse of the succeeding fluid, will be as the sine of incidence. The resistance is to be calculated as if the angle of inclination of the board was the real angle of incidence; and

then a deduction made from it in proportion to the resistance thus found, in a ratio, as the sine of incidence to radius; the deduction at 90 deg. being as 1.47 to 5.38. For instance, to a plain board, set at right angles, or 90 deg., and moving through the air at the rate of twelve feet per second, the resistance, on the supposition of its striking the air perpendicularly, and that the air is 840 times lighter than water, should be 5.38 oz. avoirdupois per square foot; but, by experiment, it is found to be only 3.91 oz., making a difference of 1.47 oz. Let the board be set at an angle of 45 deg., the resistance, on the supposition of that being the real angle of incidence, would be 2.69 oz.; but from this we must deduct a quantity in proportion to it in a ratio less than that of 1.47 to 5.38 as the sine of incidence to radius; this gives .515 for the quantity to be deducted; and 2.69—.515=2.175 oz. for the resistance. The resistance in this case has been found by experiment * to be 2.14; the difference betwixt which and that calculated by the above rule, is quite inconsiderable. Again, the resistance to the same board, moving at the same rate of twelve feet per second, and set at an angle of 60 deg., calculated by this rule is 3.076 oz.; by experiment it has been found to be 3.083 oz., being likewise as near an agreement as can be expected in such a case;† and certainly much nearer than is to be attained by those empirical rules laid down by mathematicians, which have no reference to the relation betwixt cause and effect; and which have frequently been drawn from experiments inaccurately made.

* By the author.

† A repetition of the same experiment on the resistance of the air, though managed with the greatest care, will seldom give exactly the same result, owing to the constant changes that are taking place in the density of the atmosphere.

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Hutton has given us a formula* very different from the above rule, for determining this question; and he has likewise given us a statement of a set of experiments which seem very nearly to agree with it. But it unfortunately happens that these experiments bear the most evident marks of inaccuracy. For instance, he makes the resistance to a board set at an angle of 60 deg., compared with it set at an angle of 90 deg., as 729 to 840. But the sine of the angle, which measures the breadth of the column of fluid intercepted, is only as 727 to 840; consequently, he makes the force of the same quantity of fluid acting at an angle of 60 deg. to be greater than when acting at 90 deg.; which is evidently absurd.

The greatest care and attention are always necessary in making philosophical experiments; and in few cases, more than in this we are treating of. This care is the more necessary, as people are apt to receive with undoubting confidence whatever is given to them as the result of experiment. As Hutton has not given us any particulars of the way in which his experiments were made, we cannot say how the above error may have occurred. He seems, however, to have used a whirling machine; and we would here suggest, that in using that machine, the fulcrum on which the experimental body turns should not be placed any where beyond the common centre of the machine; for if it is, the body is apt to be affected by the centrifugal force; the effects of which become commixed with, and will be mistaken for, the resistance of the fluid. The arm or lever on which the body is fixed should turn freely on the common centre of the machine; and be drawn forward against the air by a cord attached to the steel-

* Mathematical Dictionary—Article, RESISTANCE OF FLUIDS.

yard, or whatever else is used to measure the resistance. The steelyard must not be placed immediately either before or behind the experimental body; for this would affect the action of the fluid.

From the preceding views, it might at first be expected that a plain thin board should be the form of body that would suffer the greatest resistance in moving through a fluid. Yet this in fact is not the case: the resistance to the flat side of an hemisphere is considerably greater; likewise to the base of a cône, whose height is not greater than half the diameter of the base; also to the base of a pyramid of the same proportions. This must be owing to the fluid circulating more readily towards the hinder parts of these bodies, and, of course, making room for the succeeding fluid to strike the front more perpendicularly. This, indeed, is what we might not at first expect: yet, upon due reflection, we find that it is perfectly consistent with those views we have been taking of the subject. The converging or tapering part of the hind end of the body can have no effect in hindering the fluid from falling in from the sides to the vacuum behind the body, unless its length is greater than half its breadth; for if that part of the body were not there, its place would be filled up by the fluid rushing in directly from behind, which would reach the centre of the plain board as soon as that from the sides will reach the middle of the round side of the hemisphere, or the apex of the cone or pyramid. But in the case of the thin board, there is an eddy, or whirling motion in the fluid behind it, which must impede the influx of the fluid from the sides; which eddy does not exist, or at least not in the same degree, with the hemisphere, the cone, or the pyramid; consequently, the fluid from the sides falls in more readily behind them, and the succeeding fluid strikes more

perpendicularly against their front. The resistance of the air to the flat side of an hemisphere, compared with that to a round thin board of the same diameter, has been found by experiment* to be as 3.292 to 3. The difference between the resistance to the base of a pyramid, and that to a thin square board of the same size, is not quite so great.

Yet, again, the resistance to a globe is not so great as to the round side of an hemisphere. The continuity of a fluid, as we formerly observed, both increases and diminishes the resistance. It increases it by increasing the quantity of fluid put in motion; and it diminishes it by retarding the receding of the fluid from the front of the moving body, and thereby giving an obliquity of impulse to the succeeding fluid. But, upon the whole, the diminution is greater than the increase of resistance, though the proportion of these opposite effects to one another, vary with the form of the moving or resisted body. It is owing to the greater quantity of matter moved that the resistance to the round side of an hemisphere is greater than to a globe: it draws a greater current after it, and causes a greater commotion in the surrounding fluid; whilst the fluid circulates more quietly round the globe, and is left behind it in a more settled state. The resistance of the air to a globe, compared with that to the round side of an hemisphere, has been found† to be as 1 to 1.44.

It may perhaps be alleged, that as the resistance to the flat side of an hemisphere is greater than to a thin board, so the resistance to a globe should be greater than to the round side of an hemisphere, the comparative difference of form in both cases being the same, viz. in the round form of the hind part; or at least, it

* By the author. † Ibid.

may be imagined, that this should counterbalance in the globe the increase of resistance, from the greater quantity of motion communicated to the surrounding fluid by the hemisphere. To this, it may be observed, that the whole collision on a globe is small, compared with that on the flat side of an hemisphere; and, consequently, that any proportional increase upon it is the less sensible; besides, that part of a globe on which the resistance is the greatest, being the fore side of it, is at the greatest distance from the hind side; and, of course, is the less affected by the action of the fluid arising from the form of the hind part. And, on the other hand, the resistance from the quantity of matter put in motion, must be much greater with the hemisphere than the globe.

When the body moves with great velocity through the fluid, and where great precision is required in the direction, as in gunnery, it is of the utmost consequence that the body be perfectly regular in its figure. As the action of resistance depends much upon the figure of the body, so, unless that figure is uniform on both sides of the centre of its motion, it is impossible that the resistance can be uniform, or that the body can continue to move in a right line, but must be deflected to one side or other, according to the angle of inclination which these inequalities may form with the direction of its motion; or, as these inequalities may be counterbalanced by the form of the opposite side. It has been found that the same piece which will carry its bullet within an inch of the intended mark, at ten yards distance, cannot be relied on to ten inches in one hundred yards, much less to thirty in three hundred yards. This evidently shows a curvature in the track of the bullet, which must be caused by the action of the air after the bullet has been put in motion; and

has been attributed by Euler to the cause we have assigned, viz. an irregularity in the figure of the bullet. It is remarkable that Hutton should differ from Euler on this point; and attribute the deflection to a whirling motion of the bullet round its own axis, acquired by friction against the sides of the piece. Whatever effect the whirling motion of the bullet might have upon the air, the air might, by its reaction, have a similar counter effect upon it. It might affect its rotatory motion round its own axis, but we cannot at all conceive how it should affect its direct motion.

Altogether, the theory of the percussion and resistance of fluids is at present in a most imperfect state; which is evidently owing to the subject never having been analized, or reduced to its simple principles, as it ought to have been: for there certainly does not appear to be any thing in the subject utterly beyond the reach of the powers of the human mind, if employed with judgment, assiduity, and care. It is hoped the preceding view we have given of it, will lead to more correct ideas than have hitherto been entertained.

Philosophers have lately attempted to form a system entirely upon experiments by the synthetic method, laying analysis entirely aside. They seem not to be aware that the most simple experiment we can make upon this subject involves a variety of principles: of course, it must be analized before it can be properly understood. Without this, by multiplying the experiments, instead of clearing up the subject, we only increase the perplexity. Every experiment seems to contradict another, and we become involved in a labyrinth of error and confusion. It may here truly be said,

"We find no end—in wandering mazes lost."

CHAPTER III.

ROTATORY MOTION.

ROTATORY motion forms the most interesting part of mechanical philosophy; and deserves much more attention than has hitherto been bestowed on it. All pendulous motions are in a certain degree rotatory. The motion of the earth, and of all the heavenly bodies round their own axes, belongs to this department; and it will appear in the sequel highly probable that the origin of all the elastic properties of matter are to be traced to this source.

When a force is impressed upon a body in a right line passing directly through the centre of gravity, a direct progressive motion is communicated to it. But if the force is directed against the body in a right line not passing through the centre of gravity, a progressive motion with respect to other bodies is given to it, and at the same time a rotatory motion round its own axis; or a progressive and rotatory motion may be communicated to a body by a force directed against the centre of gravity, if the motion of the body is resisted by an opposing force, acting in a right line, not passing through that centre.

In examining this subject it is necessary to begin with the properties of the lever, with which it is intimately connected; and a right understanding of which,

with the use of the principle of momentum, will render rotatory motion as simple as it is curious and interesting.

Let AB (Fig. 4.) represent an inflexible rod of no weight, supported by a hinge at the point A, and in such a manner that it may turn freely. Let this rod be supported from beneath, in an horizontal position, by a prop at the other end B. Let a heavy body be placed upon this rod at the centre C; it will press upon the prop at B with only half its weight: for being supported equally by A and B, its pressure must be equally divided betwixt them. Again, let the heavy body at C be removed, and let another rod *ab*, half the length of the former, and likewise of no weight, be made to rest upon the former by means of two props at the points A and C. Let the heavy body be now laid on *c*, the centre of the second rod; here the pressure becomes equally divided betwixt *a* and *b*; but the pressure at *b* rests upon C, where it becomes again equally divided betwixt A and B; so that now the heavy body presses with only one-fourth of its weight upon the prop at B, which is the proportion that *ac* bears to AB. Now, it is evident that the circumstances of the case are not altered by supposing the two rods to be firmly joined into one; or even by supposing the upper rod to be removed altogether, and the heavy body laid on the point at *d*, immediately below *c*; the distance A*d* being equal to *ac*: therefore, if the heavy body is laid on the point *d*, its pressure on the prop B, will be to its whole weight, as A*d* is to AB; or the pressure on A will be to the pressure on B inversely as A*d* is to *d*B. In like manner it might be shown, that on whatever point of the rod AB the weight is made to rest, in whatever proportions that point divides the rod, the pressure is divided betwixt the two points A and B in the same proportions.

Consequently, the pressure on the point B, or the power or counterpressure, necessary to support that point, when there is any weight laid upon the rod, is always inversely as the distance AB is to the distance of the point on which the weight rests from the point A. Or, let the above case be reversed; and instead of the heavy body at *d*, let a prop be placed beneath, at that point; and instead of the extremities of the rod pressing on A and B in the above proportions, let two bodies, whose weights are severally equal to those pressures, be placed upon the extremities A and B; action and reaction being equal, there will still be an equilibrium of pressure on each side of the prop at *d;* and the rod with the weights upon it will rest in equilibrio.

Or again, if we suppose the prop to be removed from B, and placed at any other point, as at *c*, the end of the rod at A being still retained in its place by the hinge, the weight of the heavy body placed above will still always press upon the prop with a force in proportion to its whole weight, as the distance of that point on which it is placed is to the distance of the prop from the point A. For instance, if the prop is put at C, and the weight placed immediately above it, the prop will then have to sustain the whole pressure, and no more: but if the weight is placed at *d*, equidistant from C and A, of course C will sustain but one-half. But again, the prop continuing at C, let the weight be placed at B, the pressure upon the prop will now be double the whole weight: for, the rod being supposed inflexible, the extremity A will have a tendency to rise, but is prevented by the reaction of the hinge; which reaction must be equal to the weight at B: for the distance AC is equal to the distance CB; and the end A of the rod has the same tendency to rise that B has to descend; and as the prop at C has to sustain the pres-

sure of both, it must sustain a pressure equal to twice the weight of the body at B. In short, at whatever point the prop is put, the pressure which it sustains from any heavy body placed upon the rod, is always in direct proportion to the distance of the point on which the weight rests from the point A. It is thus that we demonstrate the properties of the lever.

Instead of supposing the heavy body to be placed upon a rod of no weight, let us suppose the body to be formed into the rod, and to be of uniform thickness and density throughout, and the end A still retained in its position by the hinge. As the power of the several parts of which the body is composed, or the pressure which they exert against, either the general inertia of the rod, or any opposing obstacle betwixt A and B, increases directly as the distance from A, so the whole power or pressure will be represented by the isosceles triangle ABD; or it may be compared to a wedge of solid gravitating matter, and the centre of gravity of the wedge will represent the centre of impulsion: but the centre of gravity of a wedge is farther from the vertex than the base, as two to one: therefore, if AB is divided by the point F, so that AF shall be to FB as two to one, the impulsion on both sides of that point will be in equilibrio; and if the rod is allowed to vibrate about A, the point F will be the centre of equilibrium. When the rod is descending, this point is neither accelerated nor retarded by the action of the parts on either side of it: for the action of these parts being opposite and equal, this point is not at all affected by them; but the rod will vibrate in the same manner as if all its matter were collected on this point. This point is hence termed the centre of oscillation.

Let the body AB be placed horizontally at rest in free space, without any other resistance to its motion

than the inertia of its parts, and pressed forward by a force acting always perpendicularly upon it on the point F; the whole body will turn round the point A which will remain at rest. If, while it is thus moving round the point A, either horizontally, or vertically as a pendulum suspended from that point, it meets with any resisting obstacle at the point F, it will impinge with its whole force against that obstacle, and the point A will not be affected by the shock.

It is a problem of very considerable importance, and particularly so in the following investigation, to find the momentum or potential velocity of a body in motion round its own centre; or to find that point whose velocity is such, that if all the matter in the body were moving with it, its rotatory power or momentum would still be the same. This point is called the centre of gyration. When the centre of gravity and the centre of oscillation are once found, the centre of gyration may easily be ascertained, as follows.—It is an universal and invariable law in mechanics, that the momentum or potential velocity which a falling body acquires, is as the perpendicular height from whence it falls; whether it descends perpendicularly, or down a continuous inclined plane, or sweeps the arch of a circle; provided always, however, that its accelerating power is all expended upon itself, and that it is accelerated by no foreign power. Whilst the pendulous body AB (Fig. 4.) is descending, the parts betwixt A and F are retarded; because they cannot descend so fast as the point F, (which they would do if unconnected with the rest, and with one another, and unresisted by the hinge at A,) without pressing the parts that lie betwixt F and B into a still greater velocity: there is a certain composition of action takes place: the parts

betwixt F and B are accelerated beyond their natural velocity, whilst those betwixt A and F are retarded. But this does not alter the power which the whole acquires in the descent: the power of the whole is equal to the sum of the descents of the several parts; which is the same as the descent of the centre of gravity multiplied by the whole weight. When the body has descended into a vertical position, so as the points C,F,B, have assumed the positions G,*f*,E, the power acquired by the whole will be the same as if it had all descended through the perpendicular height AG. But there is one point, and one only, whose descent and acquired velocity have not been affected by the action of the other parts: this is the point of oscillation F: the matter at this point has acquired the same velocity, and consequently, the same power, as if it had been allowed to descend, unconnected with the rest, either along the arch it has described, or down the perpendicular height A*f*. Now, it is evident that if we assume this as the average of the whole, and calculate the whole momentum on that supposition, we make it too much, in the ratio of A*f* to AG; and must therefore reduce the power so calculated accordingly. The square of the distance of the point sought from the point A, will be to the square of AF as AG is to AF: or the distance will be to the whole length nearly as 1.732 to 3; or as 5.77 to 10.

If a wedge is made to vibrate about its apex, the distance of the centre of oscillation from the point of suspension is to the whole length as three to four: that of the centre of gravity as two to three,—the centre of gyration nearly as 7.071 to 10: and the momentum is to what it would be if the whole body were moving with the velocity of its base as one to two,

The knowledge of this is of the greatest use in analizing the rotatory motions of cylinders and spheres.

When the body AB revolves about the point A, its motion may be divided into two; one progressive, by which its centre of gravity changes its relative position to other bodies; and the other rotatory round that centre, by which the parts change their positions with regard to one another. If it makes a complete revolution round the point A, its parts will in the same time make a revolution round the centre C. The continuance of each of these motions is quite independent of the other: the progressive motion of the centre of gravity may be stopped by a resistance directly opposed to it, and the motion of rotation continue unchanged.

The momentum of rotation is in this case one-fourth of the whole; or it is to the progressive momentum of the centre of gravity as one to three: for let the half, AC, and the whole body, AB, be divided into any number of equal parts, the quantity of matter in each of these parts of the half body, and likewise their velocities round the centre C, will be but one-half of those of the corresponding parts of the whole body round the point A; and the momenta being as the quantities of matter and the squares of the velocities, the whole momentum of the half body round the centre C will be but one-eighth of that of the whole body round the point A; and the sum of the momenta of the two parts, AC and BC, will be one-fourth. This rotatory momentum round the centre C subtracted from the whole, leaves the remaining three-fourths for the progressive momentum of the centre of gravity round the point A.

When a cylinder or sphere rolls down an inclined plane, its whole momentum, progressive and rotatory, will be the same that it would acquire in falling through the perpendicular height of the plane, or in

sliding down the plane without friction. Hence, if we know the proportions which the progressive and rotatory motions bear to one another, the progressive velocity acquired, and the time of descent are easily found.

In the case of a rolling hollow cylinder, of which the matter is supposed to be all collected upon the surface, the velocity of the surface round the centre is equal to the progressive velocity of the centre; the rotatory momentum is therefore equal to the progressive momentum, and the progressive momentum being one-half of the whole, the velocity which the centre will acquire in rolling down an inclined plane, will be to the velocity it would acquire in sliding without friction nearly as 7.071 to 10.

A solid cylinder may be supposed to consist of an infinite number of wedges, with their edges or apeces meeting in the centre. When a wedge is made to revolve about its apex, the distance, and consequently the velocity, of the centre of gyration is to that of the base nearly as 7.071 to 10: its momentum is, of course, but one-half of what it would be if moving with the velocity of the base. But the velocity of the centre of a rolling cylinder is the same as that of the circumference round the centre: consequently, the rotatory momentum of a rolling solid cylinder is to the progressive momentum of the centre as one to two; and the progressive momentum being two-thirds of the whole, the velocity which the centre will acquire in rolling down an inclined plane is to that which it would acquire in sliding without friction nearly as 8.14 to 10.

In the case of a sphere, the analizing the various motions is rather more difficult. If the sphere is homogeneous, however, an approximation may easily be obtained by supposing it divided into frustums; and if the number of these is great, they may be considered

as so many cylinders, and their momenta calculated upon the same principles. In this way it is found that the distance of the centre of gyration from the centre of the sphere is 6.34, the semidiameter being 10,—that the rotarory momentum is to the progressive momentum of the centre as 4 to 10; and the velocity which its centre acquires in rolling down an inclined plane is to that which it acquires in sliding without friction, as 8.45 to 10; and not as 7.071 to 10, as has been calculated by mathematicians.

The cylinder or sphere may be supposed to roll down the plane upon some other circle than their circumference. If this circle lies beyond the circumference of the body, it will make fewer revolutions in rolling down the plane, and, of course, the rotatory motion will bear a less proportion to the progressive; or if this circle lies within it, the body will make a greater number of revolutions, and the rotatory motion will then bear a greater proportion to the progressive. Let the hollow cylinder, for instance, be fixed within another of no weight, and of twice the diameter of the first, with the centres of the two coinciding. In rolling down the plane, the rotatory velocity of the circumference of the inclosed cylinder will now be but half the progressive velocity of the centre down the plane; of course, the rotatory momentum, which in the former case was equal to the progressive, will now be to it in the ratio of one to four. On the other hand, if the first cylinder is made to roll down the plane upon another of but half the diameter, placed within it, by way of axle, and of no weight, in descending the plane, the rotatory ve locity of the circumference of the first cylinder will now be double the progressive velocity of the centre; and the rotatory momentum will be to the progressive as four to one. If we suppose the velocity which the body would acquire in falling perpendicularly through

the height of the plane, or in sliding down the plane without friction, to be ten, and the momentum one hundred, the rotatory momentum will in this last case be eighty, and the progressive momentum only twenty, and the final velocity which it will acquire will be 4.47. Or again, let us suppose an homogeneous sphere, in the same circumstances, to roll down the plane on a circle within it, equal to half its circumference, its rotatory momentum, which in the former case of rolling upon its circumference was to its progressive momentum as four to ten, will now be to the latter as 4 to 2.5; its rotatory momentum will be 61.54, and its progressive 38.46; and its final progressive velocity will be 6.2. Without the use of the principle of momentum, the solution of these questions would be as intricate, as with it, it is extremely simple. If the sphere is not of uniform density, the case becomes more intricate: but an approximation may still be easily obtained, by supposing spheres inclosed in, or spherical shells inclosing, one another, according as the density may increase or decrease from the surface to the centre.

There is still another view to be taken of the rotatory motion of cylinders and spheres, viz. the quantity of parallel motion in any given direction. When a cylinder or sphere revolves about its centre at rest, the momentum in any given parallel direction is always one-fourth of the whole rotatory momentum: for whatever motion there may be in any one direction, there will always be an equal quantity on the opposite side of the centre and in the contrary direction; and there will also be two other equal quantities of motion, likewise on opposite sides of the centre, in contrary directions to one another, and at right angles to the two former, and with them comprise the whole rotatory momentum; so that the momentum in any one parallel direction will always be one-fourth of the whole.

CHAPTER IV.

THE ORIGIN OF THE PLANETARY SYSTEM.

"The skies are open—let us try the skies.
Forgive, great Jove, the daring enterprise."

HOWEVER often we have been told that all endeavours to investigate the theory of the creation of the world must ever be unavailing, and however many the number of failures, still the prying mind of man refuses to relinquish the attempt. It must be acknowledged that the means which we must here necessarily employ appear, at first sight, rather inauspicious. Hypotheses are always looked upon with jealousy; but hypothesis is here our only resource. It is impossible to make an experiment by creating a new system. It is equally impossible to roll back nature in that order whereby things have been brought into their present state: so that we can neither have experiment nor direct observation for our guide. The danger, however, from hypothesis is now, in the present advanced state of science, much less than formerly: from the knowledge we have of the phenomena and the laws of nature, if our hypothesis is erroneous it must soon be detected. If an hypothesis shall be hit upon that will account for many of the appearances of nature, with-

out being directly contradicted by any, it should not be despised: if it account, by a legitimate process of reasoning, for all the great phenomena and the general order of the system, it will not be easy, nor would it be just, for the mind to withhold its assent from its truth. In the hypothetical part of this inquiry the points assumed are but few, whilst the phenomena accounted for are, both in number and importance, far beyond what has ever hitherto been attempted by any theory.

It has generally been considered an insurmountable difficulty to conceive how bodies should be affected by others at a distance from them, and where there appears to be no material connection. But this difficulty will in a great measure disappear, if we consider them in their positions merely as relating to one another—as having been at first all propagated from one common source; and continuing, through the influence of one universally extended mind, to be all joined by relation into one regularly organized system; each individual forming a part of the same great whole. Viewed in this light, we shall find that by the change of the position of any one individual particle, every other in the system must necessarily be affected.

It is long since the idea of a common origin of all the parts of the planetary system was suggested by Buffon. But he was unfortunate in the manner in which he supposed the separation to have taken place, when he employed a comet to effect it: for mathematicians easily proved that had the planets been impelled from the sun by such means, they must necessarily have returned to it again in the course of every revolution.* The notion was therefore scouted, and considered as ridiculous. But although Buffon was evi-

* Buffon laboured much, but without success, to remove this difficulty.

dently wrong as to the means and the way by which he supposed the effect to have been accomplished; yet, when we consider the general grounds on which that sagacious observer of nature founded his opinion, —when we look at the planetary system, and perceive the sun moving round its own axis,—the planets moving in their orbits round the sun,—their rotatory motions, and those of the satellites round their primaries; and consider that all these motions are in one direction, and so nearly in the same plane, we must surely allow that the suggestion of these bodies having had a common origin, and their motions being produced by a common cause, merited a better fate than contemptuous neglect.

Gravitation and its laws are the great obstacles in the way of the planets having been separated from the Sun, and the satellites from their primaries. But before mathematicians had determined these obstacles to be insuperable, they might well have questioned whether matter should necessarily have had the same properties prior to the formation of the system, which it has had since the system was completed. If ever there was a period when all the bodies of this system were propagated from one mass, and had their present forms and motions given to them, that must surely be supposed to have been at the creation; and if there was a period when matter had new properties, or any properties at all, given to it, we must likewise look to the creation as the most probable for that change. If gravitation and its laws, then, did not exist prior to the creation, (and there seems to be no reasonable grounds for supposing that they did) they can form no obstacle to the several parts of the system having had a common origin.—But to proceed to our Inquiry.

Let the spherical body S (Fig. 5.) represent the Sun,

revolving about its centre C, in the direction ADBE: there will be a certain momentum on the side A of the centre C, in the parallel direction of ED; and likewise, an equal momentum on the opposite side B, in the parallel direction of DE. These motions, being equal, and in opposite directions, will counterbalance one another; and the centre C will remain at rest. Whilst the body S is thus revolving, or beginning to revolve, round the centre C, let part of the matter of which it is composed be separated from the side A, and formed into the body P, which we shall suppose to represent a planet. Let the quantity of matter and distance of P be such, that the common centre of the two bodies shall be at F, distant from C one-fourth of the radius CA. As part of the matter on the side A of the body S is now removed, the momentum on that side will be reduced, whilst that on the opposite side B continues unchanged: the centre C will therefore move somewhat forward in the direction CE; the line AB passing through the centre of the body S, and changing its position with the revolution of that body, as in the direction *a b*, will intersect the line PC in some point G, betwixt C and F; and the body P will move in the direction P*p*, parallel to its original direction before it was separated from the body S.

Let us suppose the equilibrium on each side of the new centre F to continue the same as formerly round the centre C; the momentum of HBI in the parallel direction of DE, minus the momentum of the frustum IKLH in the opposite direction ED, being equal to the former momentum of the hemisphere DBE, in the parallel direction of DE; and the sum of the momenta of the frustum KAL, and that of the body P, being likewise equal to the same in the opposite direction, parallel to ED. In order to effect this equilibrium, supposing

the semidiameter AC to be divided into twenty equal parts, the line AB, passing through the centre of the body S, in assuming the position $a\,b$, will intersect the line PC, passing through the centres of both bodies, at a distance from C equal to .176 of one of those parts; and supposing the parallel momentum of the hemisphere, when the centre C is at rest, to be =100, the momentum of HBI will be =103.337, and that of IKLH, in the contrary direction, =3.337.* On the other side of the common centre F, the momentum of KAL, in the direction parallel to ED, will be =93.405, leaving 6.595 for the momentum of the body P, to complete the equilibrium.

Let the quantity of matter in the body P be doubled: the common centre will now be removed to M, distant from C half the semidiameter AC; the line $a\,b$ will intersect PC at the point N, distant from C .97; and the momentum of OBQ, in the direction parallel to DE, will be =119.3, and that of QRTO, in the contrary direction, =19.3; on the other side of the centre M, the momentum of RAT, parallel to ED, will be =63.65, leaving 36.35 for the momentum of the body P. Hence, the momentum of P is increased from what it was in the former case, in the ratio of 36.35 to 6.59, or nearly six times, although its quantity of matter is only doubled. The square of its velocity will be increased in the ratio of 18.175 to 6.59.

Let the quantity of matter in P be again doubled, so that the common centre of it and the body S shall

* In computing the parallel momentum of a frustum of the sphere, an approximation only is here found by dividing it into a number of smaller frustums of equal thickness, and multiplying the quantity of matter in each by the square of its velocity, or the square of its parallel distance from the centre of motion. The point of intersection of the line PC by the line $a\,b$, is likewise a laboured approximation found by trials.

be on the surface of the latter, at A. The line *a b* will then intersect the line PC at U, distant from C 2.64, or a little more than one-eighth of the semidiameter AC of the body S; and the momentum of VBW in the parallel direction of DE, minus the momentum of WAV in the contrary direction, will again be equal to the original parallel momentum of the hemisphere DBE in the direction DE; whilst the whole momentum on the other side of the common centre A will now rest upon the body P, and which must likewise be equal to that of the hemisphere DBE, or 100. Here again the momentum of the body P is increased in the ratio of 100 to 36.35, and the square of its velocity in the ratio of 50 to 36.35, whilst the increase of its quantity of matter is only in the ratio of two to one.* Thus, the momentum of any given quantity of matter in the body P, is increased by the mere circumstance of its combination with other matter. In like manner it might be shown, that by reducing the quantity of matter in the body P, its velocity may be infinitely reduced to a state of rest; but every increase of matter (to a certain extent) produces an increase of velocity. The tendency of the individual particles is to remain at rest; and hence the inertia of matter. But by the action of their combination they are carried forward with a great velocity: to the action of the whole we have here opposed the resistance or the reaction of the parts; but action and reaction are equal, and directed to contrary parts; the tendency of every particle in the body P must therefore be directed towards the centre of its combination with every other particle in that body, and consequently, towards the common

* N. B.—The above calculations are made on the supposition of the body S being of homogeneous density.

centre of all. This is the cause of that mysterious phenomenon of gravitation, which has hitherto preplexed and confounded the philosophers of every age. Well may we say, in the words of Young, "small knowledge we dig up with endless toil."

It is evident from inspection of Fig. 5. that increasing the distance of P will have the same effect as increasing its quantity of matter in removing the common centre from the centre C, and consequently, in increasing the momentum of P. And it appears, too, that whether this is caused by increasing the distance or the quantity of matter in the planet P, this momentum can be no farther increased after the common centre is carried beyond the surface of the Sun S. Now, it is remarkable that the common centre of the Sun and Jupiter, which is the largest of the planets, should lie very little beyond the Sun's surface. Or again, instead of one planet P, there may be two or more; and then their several momenta should be regulated by their quantities of matter and distances: but, upon the principle of equilibrium, which we have supposed to obtain, the sum of the momenta of the several planets cannot exceed that of the hemisphere of the Sun. The equilibrium betwixt the two should, indeed, be complete: for if all the planets are brought to one side, their common centre with the Sun lies considerably beyond the surface of that body. The sum of the momenta of the planets should not, in that case, be less than that of the opposite hemisphere of the Sun, neither should it be greater; as the planets cannot be any farther accelerated, after the common centre of them and the Sun lies beyond the Sun's surface. Their momenta should therefore be a maximum, and equal to the parallel momentum of an hemisphere of the Sun: and it seems extremely probable that the two

actually do coincide; although it is impossible with our present knowledge to determine this with perfect precision. If the Sun were an homogeneous sphere, the parallel momentum of its hemisphere would be to the sum of those of all the planets nearly as three to two. But there is no reason for supposing that the Sun is an homogeneous body; or, if the matter composing it is of the same nature throughout, it must be fluid and highly elastic: for it is evident, from observation, that its surface is in a continual state of agitation or ebullition; and if it is elastic, it must increase in density from the surface towards the centre, from the pressure of superincumbent matter upon the interior strata. But in a revolving body, in which the density increases towards the centre, the rotatory momentum is not so great as in another body, containing the same quantity of matter, and whose surface is revolving with the same velocity, but whose density is uniform throughout: for by bringing the matter nearer to the centre, we bring the centre of gyration likewise nearer to it, and thereby reduce its rotatory velocity, and, consequently, the rotatory momentum of the body. From experiments that have been made to ascertain the mean density of the Earth, it is proved that it is much greater than that of the most solid mountains; of course, the Earth must be much more dense in the interior than it is at the surface. In the sequel of this inquiry, indeed, it will appear that this must necessarily be the case: the Earth having been formerly in a perfectly elastic fluid state, the weight of every stratum must have compressed, and rendered more dense, every stratum beneath it; consequently, there must be a regular increase of density from the surface to the centre. If this is the case with the Earth, we may certainly allow the same order to prevail in at least an equal degree in the Sun,

whose every appearance indicates so much elasticity. There is nothing unreasonable, then, in supposing that the rotatory momentum of the Sun may, on this account, be reduced in the ratio of two to three from what it would be if it were an homogeneous body of uniform density. And if we allow this reduction, we bring the parallel rotatory momentum of the Sun's hemisphere to a very near agreement with the sum of the momenta of all the planets. In calculating the parallel orbital momenta of the planets, we have to make a considerable addition to the progressive momenta of their centres, on account of their rotatory motions. If the side of a planet next the Sun were completely retarded, and the opposite side, of course, moving with double the velocity of the centre, as is nearly the case with Jupiter, the addition on this account would be one-fifth, being one-half of the rotatory momentum.*

It will be asked, what reason have we for supposing that there should have been an equilibrium betwixt the parallel momentum of an hemisphere of the Sun, or any of the larger bodies of the system, and the orbital momentum of the smaller body or bodies moving round it? It is answered, that we have no perfect proof that there necessarily should have been such an equilibrium: this is a principle which we are assuming without any other proof than probability. But in supposing an equilibrium, or a tendency to equilibrium, in the opposite sides of any of the departments of the natural system, there is nothing, surely, to shock our belief, but, on the contrary, the most perfect agreement with our general ideas of the laws of nature: and

* This calculation is on the supposition of the planets being of homogeneous density. But if they increase in density from the surface to the centre, as they most probably do, their momentum is here overrated.

that there actually has been such a tendency to equilibrium at the creation of the planetary system is strongly confirmed by observation of the phenomena. Our ignorance of the cause of that tendency to equilibrium is probably a consequence of our being unacquainted with the nature of the relation of the Sun to that superior system around whose centre it, with all its appendages, is supposed to revolve; and which revolution would have the same effect in causing all the parts of the solar system to gravitate towards its centre, and likewise towards one another, that the revolution of a planet round the Sun has in causing all the parts of the planet to gravitate towards the centre of that planet.*

At first sight there are many seeming objections to the preceding theory. It may be alleged that according to it the momenta of the several planets ought to be in the order of the products of their quantities of matter and distances; whereas, with regard to all those below the orbit of Jupiter, this rule is generally reversed; and likewise if we apply the theory to Jupiter and his satellites, or to any single planet, the Earth, for instance, and the central parts of the Sun within the circle described by the common centre of it and the Sun, we shall find that there is no equilibrium. A farther survey, however, of the phenomena will show that these seeming deviations are not only perfectly

* As we have not assigned any cause for this tendency in the planetary system to an equilibrium of momentum at the creation, so we are not pretending to have arrived at the *ultimate cause* of the present order of things, which probably lies in infinity; but if we have traced the *inertia*, *gravity*, and *elasticity* of matter into the motion of the system, of which it is hoped the most satisfactory evidence will appear in the sequel of this inquiry, it must then certainly be admitted that we have reached a link higher in the chain of *causation* than has ever hitherto been done.

reconcileable with our theory, but that they likewise lead to the elucidation of many other phenomena of the most curious and interesting nature. Our theory supposes a tendency to equilibrium; but a tendency to equilibrium in the whole does not necessarily imply an equilibrium in every part. It is most probable that the reason why the momenta of the several planets are not in that order which our theory would at first lead us to expect, is owing to the order in which they have come into their present positions; and that all those planets below the orbit of Jupiter had, at the period of the formation of the system, been carried to a much greater distance from the Sun, and acquired their present velocities in returning to the orbits in which they now move. With regard to the failure of equilibrium in the system of Jupiter and its satellites, the probability is, that at the creation of the system, the satellites had acted a necessary part in causing the planets to move from the Sun; and that the orbital motion of the planets, again, has been the cause of the separation of the satellites from their primaries. Jupiter having moved to such a distance from the Sun that their common centre had arrived beyond the surface of the latter, its momentum had attained a maximum; and the equilibrium of the solar system being completed, and the orbital velocity likewise of Jupiter and its gravitating tendency towards the Sun being in equilibrio, that planet could have no farther tendency to acceleration in its orbit. These circumstances had arrested the farther outward progress of the satellites of Jupiter from its body, and prevented that equilibrium which we find to obtain in the solar system.

It appears very evident that the planets had at first rolled forward from the Sun like a ball upon a plane, and that their sides next the Sun had been somehow

retarded. Let us suppose this retardation to have been complete, so that their sides next the Sun had had no progressive motion; in which case the rotatory velocities of their equators would be equal to their orbital velocities round the Sun. This nearly agrees with the condition of Jupiter, the chief of the planets, and the one by which we may conceive the equilibrium of the system would be principally regulated.

The motion of the equator of Saturn next the Sun is even somewhat retrograde; from whence we should conclude that the orbital volocity of that planet had been at one time somewhat greater than it is at present—equal at least to the rotatory velocity of its equator. This leads us into the order in which the planets have moved from the Sun.

If the orbital velocity of Saturn was at any time equal to the rotatory velocity of its equator, it must have been nearly equal to that of Jupiter, but had Jupiter preceded Saturn in their movement from the Sun, this could not have been: for in that case, from their proportional quantities of matter, the velocity of Jupiter would, according to the foregoing theory, have been two or three times that of the other; indeed, by the time that the common centre of Jupiter and the Sun arrived upon the surface of the latter, Jupiter should have acquired such a momentum as would have absorbed nearly the whole quantity necessary to fill up the equilibrium with the opposite hemisphere of the Sun. But, on the other hand, if we suppose Saturn to have preceded Jupiter, we can easily conceive that its greater distance at the time from the Sun might have so far made up for its lesser quantity of matter, that its velocity might have been equal to that of Jupiter. Then, if Saturn preceded Jupiter, we may suppose the other planets to have gone before and beyond Saturn

in the same order of their size. But Jupiter continuing to move outward from the Sun would, in time, come in for such a share of the momentum, that any disposition in the others to acquire an increase of velocity would be insufficient to counterbalance the growing power of gravitation. By the time that Jupiter had arrived at its present distance from the Sun, Saturn would have acquired a velocity more than sufficient to balance the power of gravitation: it would therefore continue to move outward, yielding part of its momentum to Jupiter, until its orbital velocity and gravitation to the Sun counterbalanced one another. The orbital velocities of the smaller planets would be insufficient to retain them at their greatest distances against the increasing power of gravitation: they would therefore return again towards the Sun, and acquire a new velocity from this power in their descent; but the power of gravitation must have been continually on the increase during the descent of these planets towards the Sun; that is to say, even at the same distances from the Sun: for had the power of gravitation been the same during as since their descent, these planets would have continued to revolve in very eccentric orbits, returning once in every revolution round the Sun, to the greatest distances from whence they had fallen; but there must have been such an increase of the power of gravitation as to admit only of their present eccentricities.

From this part of our theory we are led to a curious coincidence betwixt the velocities of the satellites and the equators of the planets to which they severally belong—a coincidence which we never should have discovered without the key afforded us by the course through which our theory supposes the planets to have

moved before they settled in their present orbits. We have supposed the planets to have been rolled forward from the Sun like a ball upon a plane. It would appear that each planet, with its attending satellite or satellites, as forming one body, has been rolled forward in like manner; the satellites, when coming immediately betwixt the planet and the Sun, being retarded in the same manner as the equatorial parts of the planet. The average velocity of the satellites of Jupiter is nearly equal to the velocity of its equator; and we have supposed Jupiter to be nearly in that position, and moving with that velocity which it at first acquired. Saturn, we have supposed, had at one time moved with a greater velocity than at present; but, ascending outward from the Sun against the power of gravity, had lost a part of its original orbital velocity; its satellites, at the same time, would ascend from it, their primary, and likewise suffer a diminution of velocity. Now, the velocity of the satellites of Saturn is, accordingly, considerably less than that of its equator. On the other hand, the Earth had been carried to a much greater than its present distance, and acquired an increase of velocity in its return towards the Sun; the Moon would at the same time be precipitated towards the Earth, and likewise acquire an increase of velocity; and accordingly, we find the velocity of the Moon round the Earth is more than double the velocity of the Earth's equator.

We have already hinted that the satellites have acted a necessary part in the movement of the planets from the Sun. It rests upon this principle, that during the outward motion of the planets from the Sun the velocities of the satellites had been equal to those of the equators of their primaries; which, in the circum-

stances of the case, must necessarily have caused a progressive motion in both from the Sun. From a due consideration of Fig. 5. it appears evident, that if there was an equilibrium of momentum betwixt a planet and an attending satellite when the latter was at any given distance from the former, and moving with the same velocity as its equator, the momentum of the satellite would be too great for an equilibrium at any less distance, if moving with the same velocity as its equator; and if there was more than an equilibrium in any one direction, the common centre of the two would have a tendency to move in that direction. Thus, when the satellite was on the opposite side of the planet from the Sun, if the momentum of the satellite was too great, it would have a tendency to move forward from the planet, and consequently from the Sun; therefore, the common centre of the satellite and planet would have a tendency outward from the Sun. Again, when the satellite came betwixt the planet and the Sun, the planet would still have a relative tendency from the satellite, and, therefore, from the Sun. The planet and satellite would in this manner continue to revolve and ascend from the Sun. And although in the systems of Jupiter and Saturn the momenta of the satellites and the hemispheres of these planets never arrived at an equilibrium, they were always tending towards it. In the case of the Earth and the Moon, these have attained nearly an equilibrium after their descent towards the Sun.

If the satellites have acted a necessary part in the movement of the planets from the Sun, we must suppose all the planets to have, or to have had, at least one. There are severals, however, that are supposed to have none: but with regard to these it may be ob-

served, that in the case of Venus it is doubtful; as several astronomers have supposed that they have actually seen a satellite attending it. Mercury is a small planet; it may have a proportionally small satellite; and, from its distance from the Earth, it may have escaped our observation. Mars seems to form an anomaly in the system, as it breaks a certain order which generally prevails in it: with the exception of it, and the four small planets lately discovered, and lying betwixt it and Jupiter, the planets decrease regularly in size outward and inward from Jupiter. According to this order, Mars should have been situated below Mercury. May not Mars have come within the sphere of attraction of some of the superior planets, and had its orbital velocity thereby accelerated so as to balance the power of gravitation, and prevent it from descending farther towards the Sun, like the Earth, Venus and Mercury? By thus coming within the sphere of attraction of some of the superior planets, it may have been deprived of a satellite or satellites; and it may at the same time have carried satellites from the planet with which it came in contact. May not those four small planets lying betwixt Mars and Jupiter have been satellites thus separated from their primaries?

Before leaving this part of the subject, we may observe a striking analogy betwixt the solar system and the systems of Jupiter and Saturn, viz. In the solar system, the largest of the planets, Jupiter, is placed at neither extremity in the order of the planets; and both outward and inward from it they decrease regularly in their quantities of matter; so in the systems of Jupiter and Saturn, beyond the largest satellite there is a smaller, and the others decrease regularly in size inward from it.

If we admit that the planets were originally rolled forward from the Sun like a ball upon a plane, which there seems no reason to doubt, this affords us the means of determining, with considerable precision, the greatest distances to which the several planets had moved from the Sun. Their orbital velocities at that period must have been nearly the same as the present rotatory velocities of their equators. But "the areas which revolving bodies describe by radii drawn to an immoveable centre of force, are proportional to the times in which they are described." Hence, the areas described in a given time by the radii drawn from the several planets to the Sun, when at their greatest distances, must have been the same as at present; and therefore, as their velocities were then less than at present, their distances must have been proportionally greater: so that to find their greatest distances, we have only to multiply their present distances by the number of times that their present orbital velocities are greater than their equatorial velocities. Thus, the present medium, and the former greatest distances of the planets Mars, the Earth, Venus and Mercury, will stand as follows:—

Names.	Number of times that their present orbital velocities are greater than their equatorial.	Present distances in Miles.	Greatest distances to which they had moved at the formation of the system.
Mars.	99⅔	144,000,000	14371,904,000
The Earth.	65¼	95,000,000	6199,605,000
Venus.	76½	68,000,000	5204,000,000
Mercury.	250⅓	37,000,000	9263,320,000

We do not pretend to give a complete elucidation of the origin of the planetary system, or of the manner in

which the power of gravity has been generated in the matter composing it: but it seems extremely probable that the above are nearly the general outlines of the order in which the several parts have been brought into their present positions; and with regard to the resolution of gravitation into the principle of action and reaction, that is supported by all the evidence that the most incredulous can desire. The greatest proof that can be demanded in support of that part of our theory is, to show that all the circumstances of the case being reversed, there is produced a directly contrary effect; that if the motion of the system is the cause of gravitation, the resistance to motion is the cause of repulsion, or elasticity. And here our theory is borne out in a very remarkable manner. Our theory supposes the rotatory motion of the planets to have been produced by the motion of the equatorial parts on the side next the Sun having been resisted when they originally moved forward from that body, like a ball rolling forward upon a plane. If their orbital momentum, or the square of their orbital velocity, is to be taken as the measure of the power of gravitation, so the square of the rotatory velocity of their equators must be taken as the measure of the power of repulsion. The power of gravitation is to be estimated by the density of the bodies of the planets; the power of repulsion by the quantity of matter it is capable of holding in a state of suspension, or perfect elasticity; such as the atmosphere of the Earth: therefore, the density of the atmosphere of each of the planets, at its surface, ought to be to the density of the body of the planet, as the square of the rotatory velocity of its equator is to the square of its orbital velocity round the Sun. Now, the atmosphere of the Earth agrees perfectly with this rule; or at least the difference is

not greater than that of the best authorities from one another with regard to the dimensions of the system, upon which the calculations must be made, or the diversity of their opinions as to the comparative densities of the atmosphere and the body of the Earth. The Moon's atmosphere, according to the above rule, ought to be upwards of 12,000 times more rare than that of the Earth; and so little appearance has the Moon of having an atmosphere, that it has been much questioned whether it has any at all. The condition of Venus comes the nearest to that of the Earth of any of the planets; and its atmosphere has all the appearance that ours might be supposed to have at the same distance. The rotatory velocity of the equator of Jupiter is nearly equal to its orbital velocity: the density of its atmosphere ought therefore to be nearly equal to that of the body of the planet; and perhaps greater than some of the solid matter at its surface. May we not therefore conclude that its belts consist of very dense, or perhaps solid matter, floating in its atmosphere? But the most beautiful and satisfying evidence of the truth of this part of our theory, if we except, perhaps, the atmosphere of the Earth, we find in the planet Saturn; the rotatory velocity of its equator is even greater than its orbital velocity; it ought accordingly to have an atmosphere of greater density than that of its body; and it appears actually to have detached solid matter from its surface, and, aided by a great centrifugal velocity, to have formed it into those rings which, ever since their discovery, have justly been looked upon as the most wonderful phenomenon of the planetary system. These rings were long supposed to form but one: it is now well ascertained there are two; and it is easy to conceive that a strong centrifugal velocity might so far overcome the cohesion of their

parts, especially when floating in so dense a fluid, as to form them into two or more.

If this theory of the cause of repulsion or elasticity is correct, it should so far set at rest that question that has been so long agitated amongst philosophers, whether heat, which is always considered as the immediate cause of repulsion or elasticity, is a body *sui generis*, or whether it is a state into which all matter may be put.

The corroborative evidences of the truth of our theory pour in thick upon us from every quarter. We have supposed the Earth, and all the other planets below Jupiter, to have been carried to a distance from the Sun far beyond their present orbits; and to have descended again towards it with an increasing gravitating power. Our theory likewise supposes every acceleration of the velocity of the planets to have been attended with an increased reaction of the parts. An increasing condensation of the bodies of the planets, and likewise of their atmospheres, must consequently have taken place: for the repulsive powers would continue the same, their being no increase of their rotatory velocities. The equilibrium betwixt the gravitating and elastic powers being thus destroyed, there must have been, during the descent of these planets towards the Sun, a continual precipitation of matter from their atmospheres, in a state in which its elasticity was overcome by its condensation. And hence the element of water, which forms so important and interesting a part in the constitution of the Earth. We cannot avoid here observing the remarkable coincidence betwixt this result of our theory and that great modern discovery in Chemistry, that water is a certain modification of air. But with regard to Jupiter, Saturn and Herschel, as they have never returned from their greatest distances, no condensation of their atmos-

pheric matter could ever have taken place; consequently, no water, no clouds, can ever belong to them; and their atmospheres, nothwithstanding their immense densities, compared with ours, must be perfectly transparent, and for ever imperceptible by us. The waters in the Moon will bear the same proportion to those on our Earth that its atmosphere does to ours. Venus, as we have already observed, comes the nearest in its circumstances of any of the planets to our Earth; and its atmosphere has the same clouded appearance which it might be supposed ours would have at the like distance.

Thus, of all the elements of which the Earth is composed, water appears to have been the last of being formed, instead of the first, as almost all cosmogonists hitherto would have had us to believe. That the above actually has been the origin of water, is corroborated by its degree of density: the part of the atmosphere from which the waters would principally be derived we may conceive would be the lower regions, next the Earth's surface; and would vary in density from that of the exterior parts of the Earth, when at its greatest distance from the Sun, to the density of the under part of the present atmosphere. The density of the great mass of it, however, would incline to the former, and would undergo a degree of condensation the same as the body of the Earth itself in descending towards the Sun. So we find that the density of water inclines much nearer to the density of the body of the Earth than to that of the atmosphere.

The above view of the constitution of the different planets will afford to the curious a wide field for speculation, with regard to the nature and habitudes of those animated beings with which we may suppose these worlds to be peopled: for it is more than proba-

ble that life will very much abound where the elastic powers are so great as on the surfaces of Jupiter and Saturn. If so, must the first of these living beings, as well as those of the Earth, have come from the Sun along with the planets, and suffered all the various changes which the matter of these has undergone until they arrived in their present situations? It is evident that if Jupiter, Saturn and Herschel are peopled, their inhabitants must be very differently constituted from those of the Earth; their atmosphere being so very dense, without any element resembling our water.

We have another proof of our theory of gravitation in the comparative densities of the different planets. It has long been observed that they severally increase in density the nearer their orbits are to the Sun. Those who reason from final causes say, that they are thus constituted in order that they may be enabled to withstand the greater heat of the Sun. But the producing cause certainly is, their greater velocity, and, consequently, the greater reaction of their parts; and their several densities are of course as the squares of their velocities. We have here a proof, too, that the planets must have been in an elastic state: for without an elastic or repulsive force to resist, we could have no measure of the condensing power, nor even proof of its ever having been employed.

There is one other proof of our theory of gravitation which is not unworthy of notice, it is from moral analogy; and it is not to be denied that there is a great resemblance, if not a perfect similarity, betwixt the laws of the physical and the moral worlds. Our theory supposes the gravitation of the parts of a planet towards one another to be the consequence of their motion from their great original, the Sun. Does not this very much resemble that moral attraction which

we observe betwixt children of the same parents, or amongst members of the same nation, or the same society, when they meet in a distant land? The very circumstance of their having moved from a common origin produces a moral attraction which otherwise would never have been felt. Again, if any of the members of this new society, after separation from their original, refuse to go along with the object of the whole, but cleave towards the mother country, we find the same repulsion takes place among them that we have observed to arise from the retardation of the side of a planet leaning towards the Sun, which is the cause of the repulsion or elasticity of the matter of the planet.

The figure of the planets forms a strong proof that at their formation they must have been in a fluid state, with a high degree of elasticity. We cannot upon any other principle than that of fluidity account for their figures corresponding so exactly with that degree of oblateness which is due to the centrifugal force of their rotatory velocities. Some philosophers, indeed, would have it that the parts of this Earth have been worn down by the rains and atmospheric influences, and carried about by the waters and the centrifugal force of the Earth's rotatory velocity, until the whole assumed its present form. But the appearances of the Earth by no means correspond with this idea. Those rocky mountainous prominences with which the surface is so generally interspersed, have certainly no appearance of ever having been dissolved in water; and if ever the figure of the Earth has been very irregular, and these prominences are the remains of what has been worn away by water, it is impossible that the parts could have been wasted down by such means, without leaving a greater variation than we find in the height of these mountains above the general surface.

The Earth, then, must have been in a fluid state, but not dissolved in water: it must have been an elastic fluid; though perhaps not perfectly elastic; that is to say, its elasticity may not have been in perfect equilibrio with its gravitating power, like its present atmosphere. We must suppose the same of all the other planets.

Our theory supposes the Earth, during the period of its formation, to have been carried to a much greater distance from the Sun than at present, and moving with a much less orbital velocity; its parts, likewise, possessing a small gravitating power; but its rotatory velocity nearly the same as at present. The centrifugal force of its rotatory velocity bearing a greater proportion to the gravitating power of the parts towards its centre, its figure must have been that of a spheroid much more oblate than at present. From the same cause, the atmosphere would partake of the same figure: it would be more accumulated towards the equator than at the poles. During the Earth's return towards the Sun, the power of the gravitation of its parts continually increasing, must have been attended with a corresponding change of figure towards one less oblate; at the same time a constant condensation and tendency towards solidity; yet the solidity never so great as to prevent the yielding, breaking, and crashing of its parts, producing all those unevennesses that its surface at present exhibits. This leads us to the remaining part of our subject—The Theory of the Earth.

CHAPTER V.

THE THEORY OF THE EARTH.

It remains to show the application of our theory to those phenomena which form the subject of what is commonly understood by "The Theory of the Earth;" that is, the nature, the origin, and arrangement of the parts of the Earth which have evidently gone through a regular process at their formation.

The Theory of the Earth has long engaged the attention of both the curious and the learned; but though many theories have been formed, they are all of them so incomplete, and many of them so absurd, that the subject is generally treated by the world rather with ridicule. There are several, indeed, of those theories which the authors themselves could never expect to be received as any thing else than amusing romances. But there are others of them of a different description. There are some of them produced by men of real science, and which contain many valuable hints, and much scientific information, that may still be useful, though at present of little value, from the theory of which they form a part being so incomplete: They resemble the detached parts of a machine which derive their value only from the part they may be made to bear in completing a whole. The arguments of Buffon for the common origin of all the parts of the

planetary system, and their having been all put in motion by one and the same power, is not to be despised because he failed in accounting for that power. If our theory, or any other, shall supply that power, then his arguments will be found entitled to our serious attention. Hutton, in his Theory of the Earth, seems to have failed in satisfying the world with regard to the agency of heat, or some such principle, (for he does not positively say it was heat) in forming and consolidating the strata of the globe: but if we can supply a principle of elasticity that must have acted in the same way as heat, his reasoning, we apprehend, will be very differently received. Those who wish to enter particularly into this part of the subject we must refer to the work itself of that author: we shall here insert from it a few extracts; and then show their perfect agreement with our theory. Indeed that is so evident that it hardly requires to be pointed out.

* "But now, instead of inquiring how far water may be instrumental in the consolidation of strata which were originally of a loose texture, we are to consider how far there may be appearances in those consolidated bodies, by which it might be concluded, whether or not the present state of their consolidation has been actually brought about by that agent.

"If water had been the menstruum by which the consolidating matter was introduced into the interstices of strata, masses of those bodies could only be found consolidated with such substances as water is capable of dissolving; and these substances would be found only in such a state as the simple separation of the solvent water might produce.

"In this case, the consolidation of strata would be

* Theory of the Earth, Vol. I. p. 46.

extremely limited; for we cannot allow more power to water than we find it has in nature; nor are we to imagine to ourselves unlimited powers in bodies, on purpose to explain those appearances by which we should be made to know the powers of nature. Let us, therefore, attend, with every possible circumspection, to the appearances of those bodies, by means of which we are to investigate the principles of mineralogy, and know the laws of nature.

"The question now before us concerns the consolidating substances of strata. Are these such as will correspond to the dissolving power of water, and to the state in which those substances might be left by the separation of their menstruum? No, far, far from this supposition is the conclusion that necessarily follows from natural appearances.

"We have strata consolidated by calcarious spar, a thing perfectly distinguishable from the stalactical concretion of calcarious earth, in consequence of aqueous solution. We have strata made solid by the formation of fluor, a substance not soluble, so far as we know, by water. We have strata consolidated with sulphureous and bituminous substances, which do not correspond to the solution of water. We have strata consolidated with silicious matter, in a state different from that under which it has been observed, on certain occasions, to be deposited by water. We have strata consolidated by feld-spar, a substance insoluble in water. We have strata consolidated by almost all the various metallic substances, with their almost endless mixtures and sulphureous compositions; that is to say, we find, perhaps, every different substance introduced into the interstices of strata which had been formed by subsidence at the bottom of the sea.

"If it is by means of water that those interstices

have been filled with those materials, water must be, like fire, an universal solvent, or cause of fluidity, and we must change entirely our opinion of water in relation to its chemical character. But there is no necessity thus to violate our chemical principles, in order to explain certain natural appearances; more especially if those appearances may be explained in another manner, consistently with the known laws of nature.

"If, again, it is by means of heat and fusion that the loose and porous structure of strata shall be supposed to have been consolidated, then every difficulty which had occurred in reasoning upon the power or agency of water is at once removed. The loose and discontinuous body of a stratum may be closed by means of softness and compression; the porous structure of the materials may be consolidated, in a similar manner, by the fusion of their substance; and foreign matter may be introduced into the open structure of strata, in form of steam or exhalation, as well as in the fluid state of fusion; consequently, heat is an agent competent for the consolidation of strata, which water alone is not. If, therefore, such an agent could be found acting in the natural place of strata, we must pronounce it proper to bring about that end.

"The examination of nature gives countenance to this supposition, so far as strata are found consolidated by every species of substance, and almost every possible mixture of those different substances; consequently, however difficult it may appear to have this application of heat, for the purpose of consolidating strata formed at the bottom of the ocean, we cannot, from natural appearances, suppose any other cause as having actually produced the effects which are now examined."

* "The arguments which have been now employed for proving that strata have been consolidated by the power of heat, or by the means of fusion, have been drawn chiefly from the insoluble nature of those consolidating substances in relation to water, which is the only general menstruum that can be allowed for the mineral regions. But there are found, in the mineral kingdom, many solid masses of sal gem, which is a soluble substance. It may be now inquired how far these masses, which are not unfrequent in the earth, tend either to confirm the present theory, or, on the contrary, to give countenance to that which supposes water the chief instrument in consolidating strata.

"The formation of salt at the bottom of the sea, without the assistance of subterranean fire, is not a thing unsupposeable, as at first sight it might appear. Let us but suppose a rock placed across the gut of Gibralter, (a case nowise unnatural), and the bottom of the Mediterranean would be certainly filled with salt, because the evaporation from the surface of that sea exceeds the measure of its supply.

"But strata of salt formed in this manner at the bottom of the sea, are as far from being consolidated by means of aqueous solution, as a bed of sand in the same situation; and we cannot explain the consolidation of such a stratum of salt by means of water, without supposing subterranean heat employed, to evaporate the brine which would successively occupy the interstices of the saline chrystals. But this, it may be observed, is equally departing from the natural operation of water, as the means for consolidating the sediment of the ocean, as if we were to suppose the same thing done by heat and fusion. For the question is

* Theory of the Earth, Vol. I. p. 74

not, If subterranean heat be of sufficient intensity for the purpose of consolidating strata by the fusion of their substances; the question is, Whether it be by means of this agent, subterranean heat, or by water alone, without the operation of a melting heat, that those materials have been variously consolidated.

"The example now under consideration, consolidated mineral salt, will serve to throw some light upon the subject; for, as it is to be shewn, that this body of salt had been consolidated by perfect fusion, and not by means of aqueous solution, the consolidation of strata of indissoluble substances, by the operation of a melting heat, will meet with that confirmation which the consistency of natural appearances can give.

"The salt rock in Cheshire lies in strata of red marl. It is horizontal in its direction. I do not know its thickness, but it is dug thirty or forty feet deep. The body of this rock is perfectly solid, and the salt, in many places, pure, colourless and transparent, breaking with a sparry cubrical structure; but the greatest part is tinged by the admixture of the marl, and that in various degrees, from the slightest tinge of red, to the most perfect opacity. Thus, the rock appears as if it had been a mass of fluid salt, in which had been floating a quantity of marly substance, not uniformly mixed, but every where separating and subsiding from the pure saline substance.

"There is also to be observed a certain regularity in this separation of the tinging from the colourless substance, which, at a proper distance, gives to the perpendicular section of the rock a distinguishable figure in its structure. When looking at this appearance near the bottom of the rock, it, at first, presented me with the figure of regular stratification; but, upon examining the whole mass of rock, I found that it was

only towards the bottom that this stratified appearance took place; and that, at the top of the rock, the most beautiful and regular figure was to be observed; but a figure the most opposite to that of stratification. It was all composed of concentric circles; and these appeared to be the section of a mass composed altogether of concentric spheres, like those beautiful systems of configuration which agates so frequently present us with in miniature. In about eight or ten feet from the top, the circles growing large, were blended together, and gradually lost their regular appearance, until, at a greater depth, they again appeared in resemblance of a stratification.

"This regular arrangement of the floating marly substance in the body of salt, which is that of the structure of a coated pebble, or that of concentric spheres, is altogether inexplicable upon any other supposition than the perfect fluidity or fusion of the salt, and the attractions and repulsions of the contained substances. It is in vain to look, in the operations of solution and evaporation, for that which nothing but perfect fluidity or fusion can explain."

Again,* "This evidence, though most conclusive with regard to the application of subterraneous heat, as the means employed in bringing into fusion all the different substances with which strata may be found consolidated, is not directly a proof that strata had been consolidated by the fusion of their proper substance. It was necessary to see the general nature of the evidence for the universal application of subterraneous heat, in the fusion of every kind of mineral body. Now, that this has been done, we may give examples of strata consolidated without the introduction

* Theory of the Earth, Vol. I. p. 97.

of foreign matter, merely by the softening or fusion of their own materials.

"For this purpose we may consider two different species of strata, such as are perfectly simple in their nature, of the most distinct substances, and whose origin is perfectly understood, consequently, whose subsequent changes may be reasoned upon with certainty and clearness. The seare the silicious and calcarious strata; and these are the two prevailing substances of the globe, all the rest being, in comparison of these, as nothing; for unless it be the bituminous or coal strata, there is hardly any other which does not necessarily contain more or less of one or other of these two substances. If, therefore, it can be shown, that both of those two general strata have been consolidated by the simple fusion of their substance, no *desideratum*, or doubt will remain with regard to the nature of that operation which has been transacted at great depths of the earth, places to which all access is denied to mortal eyes.

"We are now to prove, *first*, That those strata have been consolidated by simple fusion; and, *secondly*, That this operation is universal, in relation to the strata of the earth, as having produced the various degrees of solidity or hardness in these bodies.

"I shall first remark, that a fortuitous collection of hard bodies, such as gravel and sand, can only touch in points, and cannot, while in that hard state, be made to correspond so precisely to each others shape as to consolidate the mass. But if these hard bodies should be softened in their substance, or brought into a certain degree of fusion, they might be adapted mutually to each other, and thus consolidate the open structure of the mass. Therefore, to prove the present point, we have but to exhibit specimens of silicious

and calcarious strata which have been evidently consolidated in this manner.

"Of the first kind, great varieties occur in this country. It is, therefore, needless to describe these particularly. They are the consolidated strata of gravel and sand, often containing abundance of feldspar, and thus graduating into granite; a body, in this respect, perfectly similar to the more regular strata which we now examine.

"The second kind, again, are not so common in this country, unless we consider the shells and coralline bodies in our lime-stones, as exhibiting the same example, which indeed they do. But I have a specimen of marble from Spain, which may be described, and which will afford the most satisfactory evidence of the fact in question.

"This Spanish marble may be considered as a species of pudding-stone, being formed of calcarious gravel; a species of marble which, from Mr. Bowles' Natural History, appears to be very common in Spain. The gravel of which this marble is composed, consists of fragments of other marbles of different kinds. Among these are different species of *oolites* marble, some shell marbles, and some composed of a chalky substance, or of undistinguishable parts. But it appears that all these different marbles had been consolidated or made hard, then broken into fragments, rolled and worn by attrition, and thus collected together, along with some sand or small silicious bodies, into one mass. Lastly, this compound body is consolidated in such a manner as to give the most distinct evidence that this had been executed by the operation of heat or simple fusion.

"The proof I give is this, That besides the general conformation of those hard bodies, so as to be perfectly

adapted to each other's shape, there is, in some places, a mutual indentation of the different pieces of gravel into each other; an indentation which resembles perfectly that junction of the different bones of the *cranium*, called sutures, and which must have necessarily required a mixture of those bodies while in a soft or fluid state.

"This appearance of indentation is by no means singular, or limited to one particular specimen. I have several specimens of different marbles in which fine examples of this species of mixture may be perceived. But in this particular case of the Spanish pudding-stone, where the mutual indentation is made between two pieces of hard stone, worn round by attrition, the softening or fusion of those bodies is not simply rendered probable, but demonstrated.

"Having thus proved, that those strata had been consolidated by simple fusion, as proposed, we now proceed to shew, that this mineral operation had been not only general, as being found in all the regions of the globe, but universal, in consolidating our earth in all the various degrees, from loose and incoherent shells and sand, to the most solid bodies of the silicious and calcarious substances.

"To exemplify this in the various collections and mixtures of sands, gravels, shells, and corals, were endless and superfluous. I shall only take, for an example, one simple homogeneous body, in order to exhibit in the various degrees of consolidation, from the state of simple incoherent earth to that of the most solid marble. It must be evident that this is chalk; naturally a soft calcarious earth, but which may be also found consolidated in every different degree.

"Through the middle of the Isle of Wight, there runs a ridge of hills of indurated chalk. This ridge

runs from the Isle of Wight directly west into Dorsetshire, and goes by Corfcastle towards Dorchester, perhaps beyond that place. The sea has broke through this ridge at the west end of the Isle of Wight, where columns of the indurated chalk remain, called the Needles; the same appearance being found upon the opposite shore in Dorsetshire.

"In this field of chalk, we find every gradation of that soft earthy substance to the most consolidated body of this indurated ridge, which is not solid marble, but which has lost its chalky property, and has acquired a kind of stony hardness.

"We want only to see this cretaceous substance in its most indurated and consolidated state; and this we have in the north of Ireland, not far from the Giants Causeway. I have examined cargoes of this lime-stone brought to the west of Scotland, and find the most perfect evidence of this body having been once a mass of chalk, which is now a solid marble.

"Thus, if it is by means of fusion that the strata of the earth have been, in many places, consolidated, we must conclude, that all the degrees of consolidation, which are indefinite, have been brought about by the same means."

For a more particular view of this subject, we must refer the reader to this work of Hutton's itself: a candid examination of which, we conceive, must satisfy every one who is not swayed by prejudice, with regard to the means by which the strata of the earth have been consolidated,—that it has been by fusion, or some similar means.

There may be various reasons assigned for this part of the Huttonian theory not having been so generally received as it certainly deserves. The theory, in general, is at variance with the Mosaic account of the

creation; and this is a sufficient reason with some for rejecting it. It has also been considered an unsurmountable difficulty to procure heat or fire to produce this fusion. But Hutton does not go the length to demonstrate *how* the fusion was produced; he contents himself with the phenomena, that these must have been the consequence of fusion. "But, though the cause of fire in general," says he,* "or the operation of that power in its extreme degrees, be for us a subject involved in much obscurity, this is not the case with regard to the more common effects of heat; and, though the actual existence of subterraneous fire, as the cause of light and heat, might be a thing altogether problematical in our opinion, yet, as to other effects, there are some of these from which the action of that liquifying power may be certainly concluded as having taken place within the mineral region, although the cause should be in every other respect a thing to us unknown. In that case, where the operation or effect is evident, and cannot be disputed, to refuse to admit the power in question, merely because we had not seen it act, or because we know not every rule which it may observe in acting, would be only to found an argument upon our ignorance; it would be to misunderstand the nature of investigating physical truths, which must proceed by reasoning from effect to cause."

Again,† "In opposition to the theory of consolidating bodies by fusion, our author (Mr. Kirwan) has taken great pains to shew, that I cannot provide materials for such a fire as would be necessary, nor find the means to make it burn had I those materials. Had our author read attentively my theory he would have observed, that I give myself little or no trouble

* Theory of the Earth, Vol. I. p. 36. † Ibid. p. 235.

about that fire, or take no charge with regard to the procuring of that power, as I have not founded my theory on the *supposition* of subterraneous fire, however that fire properly follows as a conclusion from those appearances on which the theory is founded. My theory is founded upon the general appearances of mineral bodies, and upon this, that mineral bodies must have been in a state of fusion. I do not pretend to prove, demonstratively, that they had even been hot, however that conclusion also naturally follows from their having been in fusion. It is sufficient for me to demonstrate, that those bodies must have been, more or less, in a state of softness and fluidity, without any species of solution. I do not say that this fluidity had been without heat; but, if that had been the case, it would have answered equally well the purpose of my theory, so far as this went to explain the consolidation of strata or mineral bodies, which, I still repeat, must have been by simple fluidity, and not by any species of solution, or any other solvent than that universal one which permeates all bodies, and which makes them fluid.

"Our author has justly remarked the difficulty of fire burning below the earth and sea. It is not my purpose here to endeavour to remove those difficulties which perhaps only exist in those suppositions which are made on this occasion; my purpose is to show, that he had no immediate concern with that question, in discussing the subject of the consolidation which we actually find in the strata of the earth, unless my theory, with regard to the igneous origin of stony substances, had proceeded upon the supposition of a subterraneous fire. It is surely one thing to employ fire and heat to melt mineral bodies, in supposing this to be the cause of their consolidation, and another thing

to acknowledge fire or heat as having been exerted upon mineral bodies, when it is clearly proved, from actual appearances, that those bodies had been in a melted state, or that of simple fluidity. Here are distinctions that would be thrown away upon the vulgar; but, to a man of science, who analizes arguments, and reasons strictly from effect to cause, this is, I believe, the proper way of coming at the truth. If the patrons of the aqueous origin of stony substances can give us any manner of scientifical, *i. e.* intelligible investigation of that process, it shall be attended to with the most rigid impartiality, even by a patron of the igneous origin of those substances, as he wishes above all things to distinguish, in the mineral operations, those which, on the one hand, had been the effect of water, from those which, on the other hand, had been the immediate effect of fire or fusion; this has been my greatest study. But, while mineralists or geologists give us only mere opinions, what is science profited by such inconsequential observations as are founded on nothing but our vulgar notions? Is the figure of the earth *e. g.* to be doubted, because, according to the common notion of mankind, the existence of an antipod is certainly to be denied?"

There is a striking agreement betwixt this reasoning of Hutton's upon the consolidation of strata by fusion, and that part of our theory which shows that the Earth, in moving outward from the Sun, must have been in an elastic fluid state; and that it must gradually have been condensed, or cooled, in its return towards the Sun. This must have produced phenomena exactly corresponding with those which Hutton ascribes to subterraneous heat; and completely obviates the difficulty which he and other philosophers have found in procuring fire to create this heat, or to

cause that fluidity, the former existence of which is so evidently indicated by the present phenomena of the Earth.

But however satisfactory the reasoning of Hutton may be with regard to the consolidation of the strata of the Earth, he certainly completely fails when he attempts to prove that the same process of forming and consolidating new strata is still going on at the bottom of the sea. He here falls into the same error of which he accuses others; he altogether deviates from that mode of reasoning from facts, with which he sets out; and wanders into the regions of opinion and imagination. He does not attempt to shew, from experience, in digging to the greatest depths into the bowels of the earth, that there are any symptoms of that great subterraneous heat, which his theory requires for the consolidation of those strata which he supposes to be at present forming; and as to his means of procuring those materials for these new strata, by those operations that are continually going on in re-dissolving the present surface of the earth, if we should even allow him that almost eternity of time that would be necessary for producing materials to form an extensive stratum, of any considerable thickness, still he gives us no satisfactory idea how these materials, carried down in a mingled state by the rivers into the sea, are there to be separated, and formed into such distinct strata, as those which nature exhibits to us in the already formed earth. He supposes the variety of strata to be occasioned by the changing of the currents of the ocean, and the lightest materials being carried out to the greatest depths, and there quietly deposited. But the strata are certainly far too regular and uniform to be accounted for in this way.

Those theories of the Earth that have been formed

by scientific men are generally founded upon some real discovery; but a discovery not sufficiently extensive to rear a system upon. They have then recourse to an aerial or a sandy foundation for the remainder: they form a system, but it is not the system of nature: if any thing solid or substantial is attempted to be raised upon it, it immediately falls to the ground.

The solution, the suspension, the subsequent separation, precipitation, and consolidation, of the matter of which the various strata are composed, in the present state of nature, present difficulties altogether unsurmountable. But our theory gives a ready solution of all the principal difficulties. With that immense atmosphere which we suppose the Earth to have had, as it moved from the Sun, having a density at its under part equal, or approaching to, that of the body of the planet itself, and the whole matter of the Earth having then such high elastic powers, it is easy to conceive that a very great quantity of matter from its surface may have been sublimated or volatilized, and suspended in the atmosphere. That such has actually been the case, is rendered extremely probable by the analogy of Saturn and Jupiter, whose present situations very much correspond with what we suppose the Earth's to have been when at its greatest distance from the Sun. In the one of these, we see matter actually detached from the body of the planet, and formed into rings; in the other we see it floating in belts in its atmosphere. During the descent of the Earth from its greatest distance to its present position, this matter suspended in the atmosphere must have been condensed and precipitated, along with the substance of the atmosphere itself; the latter being converted into water, the former, more condensed, had sunk to the bottom, where the more solid body, or nucleus of the Earth,

being still in a state of elastic fluidity, differing nothing from that which we understand by fusion, would produce all those effects upon the precipitated matter, in forming it into consolidated strata, which Hutton attributes to subterraneous heat. Again, the infinite gradation of density in the atmosphere, with the constant change, during the descent of the Earth, in the comparative densities of the nucleus and that of the atmosphere, together with the prodigious length of time which that descent must have occupied, (many thousands of years) affords ample room for the separation or change of the matter precipitated, and the formation of that variety of strata which we find in the earth.*

The only proof which Hutton attempts to give of the existence of subterraneous heat for the consolidation and elevation of strata, and which his theory requires to be still in active operation, is that which volcanoes afford. He does not suppose that volcanoes themselves do produce those effects; but he considers them as exhibiting a redundancy of that subterraneous heat which he supposes still to exist in the bowels of the earth. He does not, however, pretend to show any connection betwixt these volcanoes and that submarine heat, which ought to be so powerful and so extensive, to produce those effects which his theory requires. If we can find no heat connected with these volcanoes in the surrounding earth, we can see no reason that there should be any at the bottom of the sea; more especially, as water is such a powerful conductor of heat, that if such heat could at any time exist, it would soon be

* When the Earth was at its greatest distance from the Sun, a single orbital revolution would occupy a period of more than four thousand of our present years.

carried off and dissipated through the whole mass of waters.

But there is no reason whatever for believing in the existence of such processes as the Huttonian theory supposes to be still in active operation, of the continual destruction and reformation of the strata of the globe. That this Earth has undergone no great change in the elevation of its strata since the deposition of the last, or the aluvial formations, is evident from a circumstance that has been too little attended to by philosophers, or rather never taken notice of at all; viz, that notwithstanding the very uneven and undulated state of its surface, there are comparatively very few places from whence the waters do not find a declivity towards the ocean. If there was any such power as the Huttonian theory supposes to be continually acting in the bowels of the earth, for elevating those imaginary new made strata, however slow in its operation, it must long ere now have deranged all those declivities by which the rain and spring waters are conducted to the sea; or rather, if such an elevating power was continually operating, such regular declivities never could have existed.

Our theory affords a remarkable confirmation of the idea suggested by De Luc, that sand has been formed by precipitation in a liquid.

Sand forms a most extensive part of the secondary strata. If the section, illustrative of the succession of the secondary formations, and the distribution of petrifactions, prefixed to Cuvier's theory of the Earth, forms any thing like a just representation of the proportions of the different secondary strata, sand must constitute betwixt a third and a half of the whole. Whatever tends, then, to illustrate the origin of this

substance must be highly interesting in the theory of the Earth.

Hutton would have us to believe that sand is a modification of gravel, "sand, in general," says he * "is no other than small fragments of hard and solid bodies, worn or rounded more or less by attrition." But sand, though not perfectly uniform either in substance, form, or size, is too much so to be compared to gravel. Hutton wished to make every thing bend to an agreement with his theory; which supposes all strata to have been formed from the dissolution of others which previously existed. We may have loose sand from beating down sand-stone; but this is not accounting for the origin of sand: for that sand-stone had evidently been in the state of loose sand previously to its consolidation. If we were to attempt to form sand from other stony substances, either by attrition, or by any of the ordinary dissolving powers of nature, we should find by far the greater part left, either in large rough lumps, or reduced to an impalpable powder; the proportion that would be left either of the size or form of sand, if it were possible to separate it from the rest, would be quite inconsiderable. But there are no such quantities, either of these large lumps or stones, or of that impalpable powder, which would have been reduced into clay, compared with the quantity of sand, and sand-stone, to warrant the idea of such an origin of sand. Whatever rounded stones and clay are found in the earth, have generally had their origin with gravel. These constitute only the last, or alluvial formations; and have been formed from solid substances by attrition in water, and by means of the other dissolving powers of nature; but sand-stone, and conse-

* Theory of the Earth, Vol. I. p. 172.

quently sand, had evidently been formed at a period long prior to these later formations.

But there is no occasion for straining the imagination to form sand in the Huttonian way; it has every appearance of a concretion, as De Luc supposes; and which our theory confirms. This renders the formation of it plain and simple. We have only to conceive the matter to have been originally sublimated, and suspended in the atmosphere, and afterwards, as the Earth descended towards the Sun, condensed and precipitated, in the same manner as we every day see rain, and falling in drops amongst the water, it would be there granulated, as metals are by a similar process. The sand, thus granulated, would, by its specific gravity, very soon sink to the bottom of the water, where it would be formed into solid strata, or sand-stone, varying according to the nature and degree of the heat or consolidating powers to which it became subjected.

If such is the origin of sand, it will naturally be asked, how do we account for that other substance which forms so great a portion of the secondary strata of the earth—calcarious matter? We must certainly draw the matter of which it is composed likewise from the atmosphere: we cannot suppose some strata to be forming of matter precipitated from the atmosphere, and at the same time other strata, alternating with these, to be forming of matter rising from beneath. Neither can we consistently ascribe the difference of the matter, of which these different strata are composed, to any thing original whilst in the atmosphere. If the peculiar nature of sand is owing to the manner in which it was precipitated, we should ascribe that of calcarious matter to the same cause. There seem just two ways in which we can suppose its precipitation to have differed from that of sand; either, it must have

been more quietly deposited in the water, without granulation, and precipitated in that state to the bottom; or, what is more probable, it had been precipitated from the atmosphere on that part of the Earth's surface which at the time was above the surface of the water; but in the course of those vicissitudes which the Earth's surface was continually undergoing, both from its change of form to a spheroid less oblate, and from the constant accessions of water it was receiving from the atmosphere, this matter would become covered with water; when there would be generated those marine productions, whose remains have so much excited the curiosity of the natural philosopher; this calcarious stratum being now covered with water, the matter next precipitated would, of course, be granulated sand; and hence those regular alternations of silicious and calcarious strata. These strata would long remain in a soft state: for the subterraneous heat, although it consolidated, would not harden them; they would only become gradually indurated or hardened as the Earth's velocity increased, and it approached its present position. The power of gravitation, or attraction of its parts, increasing with the Earth's velocity, that power had at last so overbalanced the opposite one of repulsion, that things acquired that indurated state which they now possess.

There are various other stony substances, besides the silicious and calcarious found amongst the secondary strata of the earth; but we appeal to the mineralogist if these may not be the same substances variously combined, or modified by the way in which they may have been operated upon in the great furnace of nature.

There is one species of strata, viz. coal, which Hutton ascribes to a vegetable origin. But if coal has really been formed from vegetables, (which is not at all

improbable) it must have been by a very different process from what he suggests. He supposes the vegetable matter to have been dissolved in water, and carried along to the sea, and there separated from other matter, only carrying down with it certain substances that are soluble in water; and, being quietly deposited at the bottom, he supposes it to have been there consolidated by subterraneous heat; and this process he imagines is still going on for the formation of new strata. That such a process is still going on is mere matter of opinion, and seems extremely unreasonable. Hutton makes no attempt to show any instances of such a process; indeed he supposes it to take place at the bottom of the deep sea, where it is necessarily beyond our reach. This is putting the difficulty out of sight, but it is not doing it away.

From the immense quantities of animal remains found in various strata, it is evident that animal life had very much abounded during these former periods of which we are treating; and it is certainly not unreasonable to suppose that there would likewise be very great abundance of vegetable productions: but if these have not been formed into coal, we do not see what has become of them. It may be difficult, indeed, to form any very satisfactory notions as to the particular adaptation of the order of things that then prevailed to the purposes of vegetation; but it is easy to conceive that the surface of the earth would be more level than at present; there would be more of it covered with shallow water (for all the waters of the earth had not then been formed) with high elastic powers, corresponding to what we would call great heat; and the vegetable productions would partake of that general softness which then pervaded all matter. Such a state of things must certainly have been favourable for a

great accumulation of vegetable substances. Even in the present state of nature, we often find vegetable matter accumulated in vast quantities. In flow-moss, for instance, we find it to the extent of many miles in length and breadth, and many feet in thickness. If this were covered with other strata, and then exposed to the action of a strong subterraneous heat, would it not be reduced to a substance resembling coal? We offer this as mere conjecture; but the subject is surely not undeserving of attention.

A question here naturally occurs, how could animal and vegetable life be supported during the descent of such vast quantities of matter from the atmosphere? This leads to the consideration, whether the precipitation of matter was continuous, or whether it was periodical. The precipitation we have supposed to be owing to the descent of the Earth towards the Sun; but this descent we have no reason to suppose was continuous or uniform. The Earth, it is likely, performed many revolutions round the Sun during its descent from its greatest distance. The present orbit of the Earth being an elipsis, we may certainly conclude that these revolutions, likewise, though not performed in perfect elipses, must have been of an eliptical order. The precipitation of matter from the atmosphere would take place during the Earth's motion from the aphelion to the perihelion, leaving the period of its motion from the perihelion for the production of vegetables and the propagation of animals. From the distance at which the Earth then was from the Sun, and the slowness of its motion, a single revolution must have occupied a very long period of time; so that, if other circumstances were suitable, there is no difficulty in conceiving that during the half of one of these, both animals and vegetables may have been propagated to a very great extent.

If it is considered inconceivable how animal and vegetable life could exist in one stratum, whilst another stratum beneath it was undergoing the process of fusion, we may observe, that it is not more wonderful than the different phenomena of heat and cold, which we every day perceive, We see that ice placed at the foot of a mountain will be readily dissolved into water, whilst water placed at the top of the same mountain will be as readily converted into ice. On a common hotbed the most luxuriant vegetation is going on; whilst within a few inches beneath, scarcely amounting to feet, vegetable matter is undergoing a rapid decomposition.

Philosophers have commonly distinguished the strata of the Earth into primary and secondary. Those termed secondary are found to abound in organic remains: in the primary there is no such thing to be found. This distinction perfectly coincides with our theory: we consider those as primary that constitute the original nucleus or body of the Earth. If vegetables or animals existed before this was covered with other strata, their remains may be found *upon* it, but not *in* it. We consider those strata as secondary that have been formed of matter precipitated from the atmosphere. These would, in succession, become the seat of animal and vegetable life; and in succession become covered with new matter, deposited in a soft or loose state. The matter of the strata and the inhabitants would be mixed together, and afterwards consolidated by subterraneous heat.

Hutton endeavours to refute the distinction of strata into primary and secondary. He labours to prove that those strata termed primary, are not altogether destitute of organic remains: but in this he does not seem to succeed. He finds, indeed, that they had been in a state of fusion like the other strata. He

might even have found that their fusion had been more complete: for that of the secondary strata appears to have been little more than was necessary for their consolidation; whilst the primary had flowed like subterranean lava, even amongst the secondary or horizontal strata. Our theory readily accounts for this phenomenon. The surface of a hot body cools sooner than its central parts; in like manner, the secondary strata on the surface of the Earth had become indurated, whilst the matter beneath was still fluid. Whilst the Earth was undergoing a change of figure, from a more to a less oblate spheroid, these secondary strata, originally horizontal, must have been broken and shifted from their places. The fluid matter from beneath would then be forced into every opening and crevice in every direction, rising even as high as the top of the secondary strata; and during this process, or that of its cooling, it might be formed into whinstone, basaltes, &c.

That the secondary strata have been formed from matter precipitated from the atmosphere is corroborated by this circumstance, that these strata, though the same in kind all over the Earth, vary in extent or thickness in the different latitudes, in such a proportion as our theory would assign to the quantity of matter that had been suspended in the superincumbent atmosphere of those latitudes, and from the precipitation of which these strata have been formed. Our theory supposes the Earth to have had at one period a figure much more oblate, with a centrifugal force bearing a much greater proportion to the power of gravitation than at present, and a great quantity of matter suspended in its atmosphere. This atmosphere must have acquired, from the centrifugal force, a still greater oblateness than the body of the Earth itself; and the quantity of matter

suspended in it over any given extent of the Earth's surface must have been much greater towards the equator than at the poles; and, of course, when this matter came to be precipitated, the depositions would be much more extensive in the former than in the latter. Now, this perfectly agrees with the observations of geologists, who have found the secondary strata much less extensive in the artic regions than in the lower latitudes, betwixt these regions and the equator.

We have now shown the correspondence of our theory with Buffon's in regard to the common origin of all the parts of the planetary system; with Hutton's as to the consolidation of strata; and with De Luc's idea of the origin of sand. We come now to show its agreement with the formations and displacements of strata, and the revolutions which the surface of the Earth has experienced, as marked by the varied fossil organic remains found in the successive strata. Little more will be necessary here than to transcribe Cuvier's description of these phenomena, and compare it with our theory as already delivered. This description of the phenomena by Cuvier falls in almost as readily with our theory as if it had been written on purpose to form a part of this work.

"§ 4. *First Proofs of Revolutions on the Surface of the Globe.* *

"The lowest and most level parts of the Earth, when penetrated to a very great depth, exhibit nothing but horizontal strata, composed of various substances, and containing, almost all of them, innumerable ma-

* Theory of the Earth, p. 7.

rine productions. Similar strata, with the same kind of productions, compose the hills even to a great height. Sometimes the shells are so numerous as to constitute the entire body of the stratum. They are almost everywhere in such a perfect state of preservation, that even the smallest of them retain their most delicate parts, their sharpest ridges, and their finest and tenderest processes. They are found in places far above the level of every part of the ocean, and in places to which the sea could not be conveyed by any existing cause. They are not only inclosed in loose sand, but are often incrusted and penetrated on all sides by the hardest stones. Every part of the Earth, every hemisphere, every continent, every island of any size, exhibits the same phenomenon. We are therefore forcibly led to believe, not only that the sea has at one period or another covered all our plains, but that it must have remained there for a long time, and in a state of tranquillity; which circumstance was necessary for the formation of deposits so extensive, so thick, in part so solid, and containing exuviæ so perfectly preserved.

"The time is past for ignorance to assert that these remains of organized bodies are mere *lusus naturæ*,—productions generated in the womb of the Earth by its own creative powers. A nice and scrupulous comparison of their forms, of their contexture, and frequently even of their composition, cannot detect the slightest difference between these shells, and the shells which still inhabit the sea. They have therefore once lived in the sea, and been deposited by it: the sea, consequently, must have rested in the places where the deposition has taken place. Hence, it is evident, that the basin or reservoir containing the sea, has undergone some change at least, either in extent or situation, or

in both. Such is the result of the very first search, and of the most superficial observation.

"The traces of revolutions become still more apparent and decisive when we ascend a little higher, and approach nearer to the foot of the great chains of mountains. There are still found many beds of shells; some of these are even larger and more solid; the shells are quite as numerous and as entirely preserved; but they are not of the same species with those which were found in the less elevated regions. The strata which contain them are not so generally horizontal: they have various degrees of inclination, and are sometimes situated vertically. While in the plains and low hills it was necessary to dig deep in order to detect the succession of the strata, here we perceive them by means of the vallies which time or violence has produced, and which disclose their edges to the eye of the observer. At the bottom of these declivities, huge masses of their *debris* are collected, and form round hills, the height of which is augmented by the operation of every thaw and of every storm.

"These inclined or vertical strata, which form the ridges of the secondary mountains, do not rest on the horizontal strata of the hills which are situated at their base, and serve as their first steps; but, on the contrary, are situated underneath them. The latter are placed on the declivities of the former. When we dig through the horizontal strata in the neighbourhood of the inclined strata, the inclined strata are invariably found below. Nay, sometimes, when the inclined strata are not too much elevated, their summit is surmounted by horizontal strata. The inclined strata are therefore more ancient than the horizontal strata. And as they must necessarily have been formed in a horizontal position, they have been subsequently shifted into their in-

clined or vertical position, and that too before the horizontal strata were placed above them.

"Thus the sea, previous to the formation of the horizontal strata, had formed others, which, by some means have been broken, lifted up, and overturned in a thousand ways. There had therefore been at least one change in the basin of the sea which preceded ours. It had also experienced at least one revolution; and as several of these inclined strata which it had formed first, are elevated above the level of the horizontal strata which have succeeded, and which surround them, this revolution, while it gave them their present inclination, had also caused them to project above the level of the sea, so as to form islands, or at least rocks and inequalities; and this must have happened whether one of their edges was lifted up above the water, or the depression of the opposite edge caused the water to subside. This is the second result not less obvious, nor less clearly demonstrated than the first, to every one who will take the trouble of studying carefully the remains by which it is illustrated and proved.

"§ 5. *Proofs that such Revolutions have been numerous.*

"If we institute a more detailed comparison between the various strata and those remains of animals which they contain, we shall soon discover still more numerous differences among them, indicating a proportional number of changes in their condition. The sea has not always deposited stony substances of the same kind. It has observed a regular succession as to the nature of its deposits; the more ancient the strata are, so much the more uniform and extensive are they; and the

more recent they are, the more limited are they, and the more variation is observed in them at small distances. Thus, the great catastrophes which have produced revolutions in the basin of the sea, were preceded, accompanied, and followed by changes in the nature of the fluid and of the substances which it held in solution; and when the surface of the seas came to be divided by islands and projecting ridges, different changes took place in every different basin.

"Amidst these changes of the general fluid, it must have been almost impossible for the same kind of animals to continue to live;—nor did they do so in fact. Their species, and even their genera, change with the strata; and although the same species occasionally recur at small distances, it is generally the case that the shells of the ancient strata have forms peculiar to themselves; that they gradually disappear till they are not to be seen at all in the recent strata, still less in the existing seas, in which, indeed, we never discover their corresponding species, and where several even of their genera are not to be found; that, on the contrary, the shells of the recent strata resemble, as it respects the genus, those which still exist in the sea; and that, in the last formed and loosest of these strata, there are some species which the eye of the most expert naturalist cannot distinguish from those which at present inhabit the ocean.

"In animal nature, therefore, there has been a succession of changes corresponding to those which have taken place in the chemical nature of the fluid; and when the sea last receded from our continent, its inhabitants were not very different from those which it still continues to support.

"Finally, if we examine with greater care these remains of organized bodies, we shall discover, in the

midst even of the most ancient secondary strata, other strata that are crowded with animal or vegetable productions, which belong to the land and to fresh water; and amongst the more recent strata—that is, the strata which are nearest the surface—there are some of them in which land animals are buried under heaps of marine productions. Thus, the various catastrophes of our planet have not only caused the different parts of our continent to rise by degrees from the basin of the sea, but it has also frequently happened, that lands which had been laid dry have been again covered by the water, in consequence either of these lands sinking down below the level of the sea, or the sea being raised above the level of the lands. The particular portions of the Earth also which the sea has abandoned by its last retreat, had been laid dry once before, and had at that time produced quadrupeds, birds, plants, and all kinds of terrestrial productions; it had then been inundated by the sea, which has since retired from it, and left it to be occupied by its own proper inhabitants.

"The changes which have taken place in the productions of the shelly strata, have not therefore been entirely owing to a gradual and general retreat of the waters; but to successive irruptions and retreats, the final result of which, however, has been the universal depression of the surface of the sea.

"§ 6. *Proofs that the Revolutions have been sudden.*

"These repeated irruptions and retreats of the sea have neither been slow nor gradual; most of the catastrophes which have occasioned them have been sudden; and this is easily proved, especially with regard to the last of them, the traces of which are most con-

spicuous. In the northern regions it has left the carcases of some large quadrupeds which the ice had arrested, and which are preserved even to the present day with their skin, their hair, and their flesh. If they had not been frozen as soon as killed they must quickly have been decomposed by putrifaction. But this eternal frost could not have taken possession of the regions which these animals inhabited except by the same cause which destroyed them; * this cause, therefore, must have been as sudden as its effect. The breaking to pieces and overturnings of the strata, which happened in former catastrophes, shew plainly enough that they were sudden and violent like the last: and the heaps of *debris* and rounded peebles which are found in various places among the solid strata, demonstrate the vast force of the motions excited in the mass of waters by these overturnings. Life, therefore, has been often disturbed on this earth by terrible events—calamities which, at their commencement, have perhaps moved and overturned to a great depth, the entire outer crust of the globe, but which, since these first commotions, have uniformly acted at a less depth and less generally. Numberless living beings have been the victims of these catastrophes; some have been destroyed by sudden inundations, others have been laid dry in consequence of the bottom of the seas being instantly elevated. Their races even have become extinct, and have left no memorial of

* "The two most remarkable phenomena of this kind, and which must for ever banish all idea of a slow and gradual revolution, are the rhinoceros discovered in 1771 in the banks of the Vilhoui, and the elephant recently discovered by M. Adams near the mouth of the Lena. This last retained its flesh and skin, on which was hair of two kinds; one short, fine, and crisped, resembling wool, and the other like long bristles. The flesh was still in such high preservation, that it was eaten by dogs."

them except some small fragment which the naturalist can scarcely recognise.

"Such are the conclusions which necessarily result from the objects that we meet with at every step of our inquiry, and which we can always verify by examples drawn from almost every country. Every part of the globe bears the impress of these great and terrible events so distinctly, that they must be visible to all who are qualified to read their history in the remains which they have left behind.

"But what is still more astonishing and not less certain, there have not always been living creatures on the earth; and it is easy for the observer to discover the period at which animal productions began to be deposited.

"§ 7. *Proofs of the Occurrence of Revolutions before the Existence of Living Beings.*

"As we ascend to higher points of elevation, and advance towards the lofty summits of the mountains, the remains of marine animals, that multitude of shells we have spoken of, begin very soon to grow rare, and at length disappear altogether. We arrive at strata of a different nature, which contain no vestige at all of living creatures. Nevertheless their crystallization, and even the nature of their strata, shew that they also had been formed in a fluid; their inclined position and their slopes shew that they also have been moved and overturned; the oblique manner in which they sink under the shelly strata, shews that they have been formed before these; and the height their bare and rugged tops are elevated above all the shelly strata, shews that their summits have never again been cover-

ed by the sea since they were raised up out of its bosom.

"Such are those primitive or primordial mountains which traverse our continents in various directions, rising above the clouds, separating the basins of the rivers from one another, serving, by means of their eternal snows, as reservoirs for feeding the springs, and forming in some measure the skeleton, or, as it were, the rough frame-work of the Earth.

"The sharp peaks and rugged indentations which mark their summits, and strike the eye at a great distance, are so many proofs of the violent manner in which they have been elevated. Their appearance in this respect is very different from that of the rounded mountains and the hills with flat surfaces, whose recently formed masses have always remained in the situation in which they were quietly deposited by the sea which last covered them.

"These proofs become more obvious as we approach. The vallies have no longer those gently sloping sides, or those alternately salient and re-entrant angles opposite to one another, which seem to indicate the beds of ancient streams. They widen and contract without any general rule; their waters sometimes expand into lakes, and sometimes descend in torrents; and here and there the rocks, suddenly approaching from each side, form transverse dikes, over which the waters fall in cataracts. The shattered strata of these vallies expose their edges on one side, and present on the other side large portions of their surface lying obliquely. They do not correspond in height; but those which on one side form the summit of the declivity, often dip so deep on the other as to be altogether concealed.

"Yet, amidst all this confusion, some naturalists have thought that they perceived a certain degree of

order prevailing, and that amongst these immense beds of rocks, broken and overturned though they be, a regular succession is observed, which is nearly the same in all the different chains of mountains. According to them, the granite, which surmounts every other rock, also dips under every other rock; and is the most ancient of any that has yet been discovered in the place assigned it by nature. The central ridges of most of the mountain chains are composed of it; slaty rocks, such as clay slate, granular quartz, (*gres*,) and mica slate, rest upon its sides and form lateral chains; granular, foliated limestone, or marble, and other calcarious rocks that do not contain shells, rest upon the slate, forming the exterior ranges, and are the last formations by which this ancient uninhabited sea seems to have prepared itself for the production of its beds of shells.

"On all occasions, even in districts that lie at a distance from the great mountain chains, where the more recent strata have been dug through, and the external covering of the Earth penetrated to a considerable depth, nearly the same order of stratification has been found as that already described. The crystallized marbles never cover the shelly strata; the granite in mass never rests upon the crystallized marble, except in a few places where it seems to have been formed of granites of newer epochs. In one word, the foregoing arrangement appears to be general, and must therefore depend upon general causes, which have on all occasions exerted the same influence from one extremity of the Earth to the other.

"Hence, it is impossible to deny, that the waters of the sea have formerly, and for a long time, covered those masses of matter which now constitute our highest mountains; and farther, that those waters, during

a long time, did not support any living bodies. Thus, it has not been only since the commencement of animal life, that these numerous changes and revolutions have taken place in the constitution of the external covering of our globe. For the masses formed previous to that event have suffered changes, as well as those which have been formed since; they have also suffered violent changes in their positions, and a part of these assuredly took place while they existed alone, and before they were covered by the shelly masses. The proof of this lies in the overturnings, the disruptions, and the fissures which are observable in their strata, as well as in those of more recent formation, which are there even in greater number and better defined.

"But these primitive masses have also suffered other revolutions posterior to the formation of the secondary strata, and have perhaps given rise to, or at least have partaken of, some portion of the revolutions and changes which these latter strata have experienced. There are actually considerable portions of the primitive strata uncovered, although placed in lower situations than many of the secondary strata; and we cannot conceive how it should have so happened, unless the primitive strata, in these places, had forced themselves into view, after the formation of those which are secondary. In some countries, we find numerous and prodigiously large blocks of primitive substances scattered over the surface of the secondary strata, and separated by deep vallies from the peaks or ridges whence these blocks must have been derived. It is necessary, therefore, either that these blocks must have been thrown into those situations by means of irruptions, or that the vallies, which otherwise must have stopped their course, did not exist at the time of their being transported to their present sites.

"Thus we have a collection of facts, a series of epochs anterior to the present time, and of which the successive steps may be ascertained with perfect certainty, although the periods which intervene cannot be determined with any degree of precision. These epochs form so many fixed points, answering as rules for directing our inquiries respecting this ancient chronology of the Earth."

Cuvier here describes the earliest of these secondary formations as having, from their appearance in extent and thickness, been deposited in a tranquil sea. Our theory supposes these formations to have begun to take place when the Earth was beginning to return from its greatest distance from the Sun towards its present position. Its surface being then perfectly smooth, the first deposits would be uniformly extended over its whole surface; and the quantity of matter suspended in the atmosphere being then great, the depositions from it would be the more copious. This, and the length of time employed in the first orbital revolutions, would more than compensate for the slowness of the Earth's descent towards the Sun, which, and its consequent acceleration, we have supposed to have been the cause of the general condensation of the Earth, and the precipitation of matter from its atmosphere. The matter thus precipitated would be gradually condensed into regular solid strata; but the Earth continually changing its form to one less oblate, these strata must have been broken in a variety of ways, and shifted from their original positions; this must have presented a less even surface for receiving the succeeding depositions from the atmosphere; these again, in their turn, would be consolidated, broken, and shifted from their places. Thus, in general, every succeeding stratum would be less regular than the one which preceded it; and

though the total quantity of water would be continually increasing, the surface of the Earth would be less regularly covered by it: whilst some parts of the broken strata would be sinking beneath the waters, others would be rising above them, forming continents and islands. In such general commotions it is impossible that the strata could have retained their original horizontal positions: whilst one edge would be sinking beneath the general surface, its opposite side would be rising above it and forming a mountain. It is natural to imagine, that in some cases, the rents in the strata would be very extensive; and hence the extent of some ridges of mountains.

As the quantity of matter suspended in the atmosphere became gradually reduced, the depositions would be more and more limited; corresponding with the observation of Cuvier, that the later formed strata are not so thick as the earlier formations.

We must here observe a discrepancy in the reasoning of Cuvier in the above extract. "Life" says he "has been often disturbed on this Earth by terrible events—calamities which, at their commencement, have perhaps moved and overturned, to a great depth, the entire outer crust of the globe; but which, since these first commotions, have uniformly acted at a less depth, and less generally." And afterwards (which is certainly the more correct opinion) he says, "but these primitive masses have also suffered other revolutions, posterior to the formation of the secondary strata, and have perhaps given rise to, or at least partaken of, some portion of the revolutions and changes which these latter strata have experienced." Wherever the secondary strata have been moved or elevated after their first formation, it evidently has not been by a power acting immediately on these strata, without

moving at the same time those beneath: for had that been the case, a vacancy must have been left where that power operated. But no such vacancies appear; and the whole appearances are such as can only be accounted for by the simultaneous elevation of all the strata beneath, even down to the original nucleus, termed the primary strata. This, being still in a state of fluidity or fusion, would invariably fill up the space deserted by the elevation of the secondary strata; and hence, the deeper the strata, the more numerous the changes or elevations they have suffered, and the more vertical the positions in which they are now found.

No other cause than that which our theory assigns could have acted so generally over the whole Earth, or have agreed so perfectly with that observation of Cuvier's. "In one word," says he, "the foregoing arrangement appears to be general, and must therefore depend upon general causes, which have on all occasions exerted the same influence from one extremity of the Earth to the other."

There is evidently the most striking agreement, too, betwixt our theory and the above view given by Cuvier, of the gradations in the fossil organic remains observable in the secondary or horizontal strata of the Earth. "Amidst these changes of the general fluid it must have been almost impossible for the same kind of animals to continue to live;—nor did they do so in fact. Their species, and even their genera, change with the strata." It is impossible that animals, at all corresponding to the greater part of those which at present inhabit the Earth, could have existed when these secondary strata first began to be formed. That universal softness which then pervaded all matter could never produce that firmness of texture which their present bodies possess; still less could it afford

any thing corresponding to those bones which are at present so essential to the support of their frames; or even if such animals had been formed, they could not have had a firm ground on which to tread. If any kind of animals, at all resembling those which are at present on the Earth, had then existed, they must have been of the softer species. According to the above account of Cuvier, they had been shell fish: but the shells of even these could not have had that hardness which they now possess. Those shells which they appear to have had must have been afterwards hardened along with the strata in which they were enveloped. And such appear, from the preceding view of Cuvier, to have been the first inhabitants of the Earth—animals which seem to have had almost no resemblance to any of those which at present exist. But according to the gradual and successive changes which took place in the strata, and the general constitution of nature, so the animal creation had undergone regular and successive changes, coming gradually nearer and nearer in similitude to the present inhabitants of the Earth, till they at last arrived at their present state of perfection.

Cuvier supposes there was a time when there were no living creatures on the Earth; because he finds strata which had been in a fluid state, but which contain no animal remains; and these strata, from their nature, he imagines must have been formed in a fluid. It is remarkable that the generality of philosophers can never conceive matter to be reduced to fluidity without the agency of other matter originally or essentially fluid. Do we not every day see metals, glass, and other kinds of matter, rendered fluid by fusion? And this was certainly the original state of the nucleus of the earth; and these strata in which Cuvier can

find no animal remains had at one time formed the exterior of this nucleus. That animals should have lived *in* such strata appears impossible; whether any had lived *on* them may be doubtful; though we should rather imagine that there had been living animals there, but in the lowest state. The fusion of the original nucleus might not be inconsistent with the existence of animal life upon its surface; it might be calculated to destroy life and consolidate strata only when covered with other matter. Yet, if we trace backwards through those various stages by which living creatures have been brought to their present state of maturity, we find them degenerating so very fast, that if any had existed upon the primary nucleus they must have been in the very lowest state of animal life. However wonderful it may at first sight appear, that the race of animals from so low a state should have been progressively brought to what they now are, it is perfectly conformable to the ordinary course of nature; we see nothing brought to perfection at once, but by degrees. The discoveries in comparative anatomy show the most evident, though often distant, resemblance betwixt the corresponding parts of man and other land animals. This greatly strengthens the idea, that there has been something common in their origin; and that those diversities in genera and species are owing to the peculiar circumstances by which they have been affected in their different stages of maturation.

Have land and water animals been originally distinct? Or, may the two be connected by means of the amphibious kinds? Comparative anatomy finds little or no resemblance between them; and as it is evident that there must always have been water on the Earth since the commencement of its condensation, when the creation of animals perhaps first began, or at least when

there would be a remarkable epoch in their formation; and as it is likewise extremely probable that, in the midst of all its revolutions, some parts of the Earth would always be above the water, on which would be preserved a part of the land animals from the great destruction that must frequently have taken place; we should therefore conclude, that there has always been a distinction betwixt land and water animals from the beginning. Or, perhaps, by going still farther back, we may trace the origin of both kinds of animals to the atmosphere, which might long afterwards continue to be the principal element of those whose progeny were ultimately destined to inhabit the land.

It seems to require one kind of talent to collect and prepare materials, and another to form a system of them; to effect both is perhaps beyond the ability of any one man. However just may be Cuvier's observations on the shifting of the earlier, and the subsequent formation of later strata; and however much we may admire his investigation of fossil organic remains; yet the way in which he supposes these things to have been brought about is altogether inadmissable. In the extract which we have made from his work, he supposes the various changes that had taken place in the living animals, whose remains are enclosed in the different strata, had been owing to a change in the nature of the fluid in which they had lived; but, in a subsequent part of his work, he says, "I do not pretend that a new creation was required for calling our present races of animals into existence. I only urge, that they did not anciently occupy the same places, and that they must have come from some other part of the globe." Had the changes, observed in the animal remains, been merely changes of species or genera, this might have done well enough to have supposed them brought from

other parts of the globe; but they have evidently been animals of an entire different nature—animals that must have required, not merely a change in the *position*, but a change in the *nature*, of the fluid in which they lived. To suppose the animals to have been brought from a different part of the globe, is merely shifting the difficulty; it is not solving it. The investigations that have been made in the strata, show that these are generally the same all over the Earth. When time shall have afforded opportunities for investigating the animal remains in the corresponding strata in all the different parts of the Earth, it is more than probable that these will likewise be found, not indeed perfectly similar, (for we find very different animals at present in the different quarters of the globe,) but such as must have required similar elements for their existence. We shall find them beginning with the simplest state of nature in the earliest formations, and going through the various stages of refinement, till we come to those at present inhabiting the surface. Thus, the same progressive improvement will be found to have taken place in all the different quarters of the globe. But this general improvement could never have been brought about by the mere shifting of animals from one part of the Earth to another; it must have been produced by "general causes, which have on all occasions exerted the same influence from one extremity of the Earth to the other."

Again, with regard to the phenomena of the various strata of the Earth, Cuvier is of opinion that these afford evident proofs of frequent irruptions of the sea—that these irruptions have been caused by the depression of the land, and the elevation of the bed of the ocean—that these depressions and elevations had frequently occurred; indeed, according to his view of the subject,

they must have occurred as often as there are strata which indicate having at some period formed dry land. But if these alternate elevations and depressions had happened so frequently to the surface of the Earth, after it had been completely indurated, irregular as it is, it would have been much more so; the continents would have been entirely broken to pieces. There is still one remarkable instance of regularity observable on the surface of the Earth, which we have already taken notice of, viz. that there are comparatively very few spots from whence there is not a declivity towards the sea. But this regularity could never have existed after that order of things which Cuvier supposes to have prevailed. The whole continents must in that event have been divided into islands, or at least much more interspersed with lakes or pools of water than they at present are. There are certainly very evident proofs in many of the strata beneath the Earth's surface of their having been at one time covered by the waters of the sea; but the appearance of the present surface of the Earth does not all correspond with those ideas which we would naturally form of the bed of the ocean. It exhibits indisputable proofs, however, of having been at some time or other subjected to the action of strong currents of water; at least in all the lower grounds. Hence, it is evident, that, to account for the phenomena, we must look for something else than simple depression and subsequent elevation from the bottom of the sea. We shall here give Cuvier's ideas, with regard to what he reckons the last revolution, in his own words:—

"*Concluding Reflections.*

"I am of opinion, then, with M. De Luc and M. Dolomieu, That, if there is any circumstance thoroughly

established in geology, it is, that the crust of our globe has been subjected to a great and sudden revolution, the epoch of which cannot be dated much farther back than five or six thousand years ago; that this revolution had buried all the countries which were inhabited by men and by the other animals that are now best known; that the same revolution had laid dry the bed of the last ocean, which now forms all the countries at present inhabited; that the small number of individuals of men and other animals that escaped from the effects of that great revolution, have since propagated and spread over the lands newly laid dry."

Cuvier here supposes that this imaginary old continent was destroyed, and the new one formed, by one and the same revolution. But had such an event occurred in such a way, there must not only have been an immense destruction in the number of animals, but many species, and even genera, of the old continent, must have been extinguished; indeed we cannot conceive how any could have escaped, except a few that might fly to the tops of the highest mountains, which, we shall suppose, form those islands scattered through the sea: we do not see how it is possible that any could make their escape to the new formed land. If the continent of America, for instance, were just now to be buried in the sea, except the tops of a few of the highest mountains, the coasts and all the low grounds would be first covered with the waters, and all the inhabitants drowned, but a few that might fly to these mountains, where they would be preserved as on so many islands; but we do no see how any could escape to the continents of either Europe, Africa or Asia.

Again, let us suppose a new continent to be rising in the midst of the Atlantic ocean. It is impossible to conceive how this continent is to be stored with ani-

mals, except from the few small islands that are in it: it could get none from the old continent. This new continent must begin to rise above the waters either on the coasts or in the central parts. If the latter, which would be the more natural way, as the central parts are ultimately destined to rise to the greatest height, then there would always be a great body of water betwixt this new rising continent and the old, which would completely prevent all access of animals from the one to the other. Or, if we suppose the coasts of the new continent, and which we shall grant to be immediately adjoining those of the old, to rise first above the waters; then the whole mass of water would be accumulated upon the central parts; and as these afterwards rose, it would be poured over the coasts, drowning every living creature that had taken refuge there from the wreck of the former land. But even making every allowance possible for the escape of animals from the old continent to the new, unless we bring in on every occasion an Ark and a Noah, with miraculous powers, it is evident that many kinds must have been completely destroyed; or rather, comparatively, few kinds could have been saved. It certainly will not be asserted, however, on comparing the remains found in any of the strata, which had constituted the former surface of the Earth, with the number of kinds at present existing, that there is any probability of such complete destruction having ever taken place. Whatever difference there may be betwen the fossil remains found in these strata, and the animals at present in existence, there is certainly no diminution in the numbers of genera or species. They have been changed but not destroyed.

Independent of all this reasoning, there is a remarkable circumstance mentioned by Cuvier himself, which

shows that there had been no intercourse of quadrupeds between America, and either Europe, Asia or Africa, till America was discovered by the Spaniards. "Thus" says he, "when the Spaniards first penetrated into South America, they did not find it to contain a single quadruped exactly the same with those of Europe, Asia or Africa." This most satisfactorily proves that there had been no connection between the new continent and the old, from the first creation of quadrupeds till it was discovered by Europeans; at least no such connection as permitted the conveyance of quadrupeds from the one to the other. But had these present continents been stored with quadrupeds from former countries, existing either in the Atlantic or Pacific oceans, it is impossible but some of the same kinds must have escaped to both sides. This is a very important point in forming a theory of the Earth; not only as it shows that there has been no connection betwixt the two continents since the creation of quadrupeds, but likewise as it is possible to trace whether, in the corresponding under strata in the two continents, there is the same difference in the remains of land animals as has been found upon the surface. If there is, we should imagine these continents never to have been joined since their first separation by water; or, if these remains shall be found to be the same in any of the under corresponding strata, it will show at what period the separation had taken place. In the mean time, we should consider the separation to have taken place previous to the creation of those animals in their most simple state, from whence the present race of quadrupeds have been produced. When Cuvier considers it as one of the circumstances most thoroughly established in geology, that the present bed of the ocean had at one time formed the habitation of men and

other animals, he offers no positive proof of it; but, like many other philosophers who consider the present quantity of water to have always existed on the Earth, he could find no other means for covering the present land but by depressing it and elevating the present bed of the sea. But our theory accounts for this wonderful phenomenon by more simple means. Instead of those numerous, violent, and alternate elevations and depressions, we conceive all the phenomena may be accounted for, and, much more satisfactorily, by elevations alone: elevations not continuous, however, but periodical, as the stratification near the surfaee would lead us to expect. We have supposed that the Earth in descending from its greatest distance from the Sun, towards its present orbit, had moved in an orbit of an eccentric order; we have supposed the precipitation of water from the atmosphere, as well as of that opaque matter of which the horizontal or secondary strata are formed, to have taken place when the Earth's velocity was accelerated, or during its motion from its aphelion to its perihelion; but while moving in the other half of its orbit, on the contrary, we considered that this precipitation from the atmosphere would cease. In the course of time, as the external crust began to cool and harden, and the figure of the Earth to change, these strata, as formerly observed, must have been rent and broken in a variety of places, and the matter of the original nucleus forced up through the rents, elevating the adjoining edges of the horizontal strata. As it is the nature of heat to propagate itself most readily where the matter is homogeneous, and to rise where matter is accumulated in heaps, so the central heat would ever after act with the greatest force upon the primary matter forced up through these original rents. This central heat would likewise act with the greatest efficiency

whilst the Earth was moving from its perihelion to its aphelion, as the velocity of the Earth, and consequently the power of gravitation, was not increasing, but probably retrograding, during that period. In this part of the Earth's orbit, then, elevations would take place, and dry land would be formed; whilst in the other, or the descending half of its orbit, the lower parts of this dry land would again be covered with water, and a stratum of other matter precipitated from the atmosphere. Thus, we have a continued alternation of elevations and coverings of water and other matter, without any sinking or depressions of the land; at least any general depressions, or such as would extend to whole continents; for in the general convulsions there must have been many partial depressions of strata. Let it always be understood, we mean here, relative elevations and depressions, as the whole mass of the Earth was, during this period, condensing or concentrating into less bulk.

According to this view of the subject, the bed of the deeper parts of the ocean have never formed dry land since the beginning of the formation of water. Those parts, indeed, that lie towards the shores may have been above the waters at the earlier periods of the depositions from the atmosphere; but it is not likely that even these have ever formed dry land since the formation of the Earth was completed. It is extremely probable, that many parts of what now forms the same continent had at one time been separated by water, until connected by the formation of secondary strata, and their subsequent elevation. The complete separation of the continents from the beginning, accounts, not only for the difference in the quadrupeds which inhabit them, but likewise for the differences

observable in the figure and complexion of the human species in the different quarters of the globe.

We have another proof of our theory in a remarkable phenomenon that has much excited the wonder and curiosity of naturalists, viz. those immense collections that are found in the coldest regions of the Earth of the remains of animals whose species are now utterly incapable of living there, and are at present found only in the tropical regions. This receives a ready solution from that great original heat which we have assigned to the primary nucleus of the Earth; and which must have rendered every part of it sufficiently warm for every species of animal and vegetable life.

That the polar regions had been warmed by original subterranean heat is corroborated by another circumstance—that towards the south pole the cold is so much greater than towards the north. Water is a more ready conductor of heat than earth is; and by the greater quantity of water at the south pole than at the north, more of the heat has been carried off by evaporation into the atmosphere. Did the Earth derive its heat absolutely from the Sun, this difference of heat in these opposite parts ought not to exist, or rather it should be reversed; for water being such a ready conductor of heat, it should convey the greater quantity from the torrid zone to the south pole.

From the phenomena on the Earth's surface being so much more open to our inspection, it might have been expected that philosophers would have attained to more correct knowledge with regard to these than the interior strata; but it does not appear that they have. They present us with nothing on this subject but the most vague ideas and conjectures about deluges, and irruptions of the sea, and revolutions and

catastrophes. They have evidently consulted their imaginations more than either their bodily or their mental eyes; otherwise they might have discovered much regularity even in the midst of all the seeming confusion that prevails on the Earth, and evident indications of the causes that have operated to produce the present state of things. But we need not be surprised at the inconsistencies and absurdities of their theories, so long as they endeavour to make them accord with the ridiculous story told, or said to have been told, by Moses about an universal deluge.

It is hardly possible to penetrate any of the lower parts of the Earth's surface, where it does not consist of solid rock, without perceiving evident proofs of the former action of water. Hence, it is easy to account, without either tradition or revelation from heaven, for the vague ideas of a deluge that have been entertained by men in all countries and all ages. But the acute examiner must very soon perceive that these appearances are by no means such as can be accounted for by a mere covering of the Earth with water. The water must have been in motion, and moving with great rapidity. We see great quantities of gravel, that has evidently been formed from the consolidated strata, broken and worn down by attrition in water. We see immense quantities of clay, consisting of the matter that has been more finely worn down by the friction in forming the gravel. We find great quantities of sand; some by itself, forming banks or horizontal strata, and much of it mixed with other matter. It is possible that the state of part of this sand may never have been changed since its first formation; but from the great quantities of sand-stone found amongst the gravel, a great part of this loose sand must certainly have been produced in the forming of this gravel. All these phenomena indi-

cate the former existence of strong currents of water on the surface of the Earth, where nothing of the kind is now to be seen.

But we have the strongest evidence, too, that the direction of these currents, at least in our northern latitudes, had been from west to east. When we examine the form of the hills situated in the vallies or low grounds, we find that they are all more elongated from west to east than from south to north;—we find, too, that their west ends are all more precipitous than their east. From this we must naturally infer that all the soft and loose materials had been swept away from their west end, whilst at the east they had found shelter from the violence of the current, and there formed a bank sloping gradually down to the level of the plain. For effects so uniform and extensive we can conceive no other cause, than that the waters of the ocean had moved over these lower parts of the Earth's surface from west to east, with very great rapidity, and for a very long period of time.

We have a remarkable proof of the former existence and direction of this current of water in the tract of the Caledonian Canal; besides many other instances, even in Great Britain, where the waters of the sea have run across the island; but there, from the particular nature of the channel in which the waters had run, being lined on both sides with very high mountains, the current had been very much concentrated, and consequently the effects it has left behind are the more striking. The whole of this tract, from sea to sea, consists of a continued chain of lochs or lakes, whose surfaces are very little above the level of the sea; and which are separated from one another only by small necks of land; consisting almost entirely of gravel of the coarsest nature perhaps any where to be found.

It is this last circumstance, indeed, that has proved such an obstruction in the completion of the above canal; a work which otherwise seemed of very easy accomplishment; but from the coarseness and porousness of this gravel, it has hitherto been found impossible to make the banks sufficiently retentive of water. The most superficial view of these lochs, separated as they are only by small portions of gravel, must satisfy any one that there has been here at one time a violent current of water from the one sea to the other. And that the direction of the current had been from west to east, is evident from this circumstance, that the farther east the gravel, that had been carried along with the current, becomes finer and finer, and more mixed with sand; the coarser gravel, being more capable of resisting the action of the water, had first subsided, whilst the finer parts were carried farther along, till we come to the eastern estuary, where the water having room to spread, and the violence of the current, of course, had abated, it has there deposited almost pure sand; and the whole soil there, for several miles from the coast, is of so light and sandy a nature, that in many cases it is blown with the wind; and that to such an extent, indeed, as to have attracted the attention of the Highland Society of Scotland, which has been offering premiums for the best mode of covering it with a sward of grass.

But however evident may appear the former action of water on the lower parts of the Earth's surface, when we ascend the mountainous districts we perceive a very different order of things: it does not appear that ever these parts have been covered with water since they were finally elevated to their present situations. Some philosophers, indeed, have amused themselves with the idea, that all these vallies and hollows

which separate the different mountains are the beds of so many former rivers. But this is a mere vagary of the imagination: it is impossible in the present situation of these vallies to conceive a source from whence water could come to form such rivers. Before we can suppose this at all possible, we must suppose these mountain districts, and the present plains below, to have interchanged conditions. The mountain districts must then have formed the vallies, and the present vallies must have formed the higher grounds. It is evident, however, to the most superficial observer, that the general appearance of the mountain districts is altogether different from that of the plains below; and it is as evident that these plains have never constituted mountains; and the more narrowly we examine them the greater does the difference of these districts appear. But without proceeding to prove by many arguments that this interchange of conditions betwixt the mountains and the vallies has never taken place, we shall merely offer two proofs, which seem never to have been taken notice of by philosophers, but which seem perfectly decisive of the point. In the first place, in the mountain districts we do not find in the individual mountains that uniformity of figure observeable in the hills in the plains;—we do not find them more elongated from west to east than from south to north;—we do not find them more precipitous in their west than in their east ends, and tapering off with a bank of alluvious matter at their eastern extremities; which things are almost universally observeable in the hills situated in the plains. In the second place, in the mountain districts we find no extensive alluvious formations, as in the plains; no extensive beds or banks of sand or gravel; or, generally speaking, we find no gravel at all. In the little vallies among the mountains

we discover many small alluvious formations deposited by the rivulets; but in these, although we find abundance of small stones having their sharper points worn off by friction, we see none completely rounded into gravel. Neither is it to the hardness of the materials that this can be ascribed; for we find these appearances amongst mountains where the materials are as soft and loose as in the plains.

In certain extensive mountain districts, indeed, where the rivulets are collected into rivers, and where their action is more violent, and the stones moved along with them have been carried over a longer course; there, we may find them completely rounded into gravel. On the sides of extensive lakes, too, we find perfect gravel, formed by the constant fretting action of the waves breaking upon their shores. In both these cases, while we see the gravel, we likewise see the causes that have produced it; but we see none in those imaginary deserted beds of former rivers.

On the other hand, when we examine those plains in the lower grounds that have been overrun with the waters of the sea, we find the whole surface covered with clay, sand, gravel, or other alluvion, excepting here and there where it is broken through with rock forced up from beneath, generally forming hills or mountains; though frequently rising only to the common level of the surface, where the decomposing powers of the atmosphere and water have reduced part of it to a mould, fit for the purposes of vegetation. Every stone found amongst the alluvion in these plains affords proofs of having been rolled and rounded by attrition in water; and many of them are of such a size as to show that the power by which they had been carried along must have been very great.

The height, too, to which the waters had risen in

these plains may be easily discovered: by observing the loose stones lying upon the surface we at once see whether they are rounded or not; and hence we can readily trace the boundary betwixt the plains that had been last overrun with the waters of the sea, and the mountain districts which had not. It affords a striking instance of the carlessness with which philosophers have made their observations that this circumstance should never before have been discovered; at least by moderns.

The ancient Chinese seem to have made more correct observations with regard to the action of the waters upon the Earth than our modern Europeans have done. The Chou-King, the most ancient of the Chinese books, and said to have been compiled by Confucius, begins the history of that country with an emperor; Yao, whom, in the following terms, it represents as having let loose the waters upon the Earth: "Having raised himself to heaven, Yao bathed the feet of the highest mountains, covered the less elevated hills, and rendered the plains impassable." *

Modern philosophers pique themselves on their superior penetration in discovering marine organic remains in the more elevated strata, even in the high mountainous districts; and many of them have considered this as a proof of the Mosaic account of the deluge, and of the waters of the sea having covered those parts. But no mind, that is not perfectly warped by prejudice, could ever view these phenomena in this light; they are altogether different from what we should expect from a mere covering of water. These remains must have been deposited there previous to the consolidation of the strata in which they are found;

* Cuvier's Theory of the Earth.

and the consolidation of these strata must have taken place previous to their elevation to their present positions. We find some men, indeed, eminent for their philosophical attainments, seriously endeavouring to prove that these elevated and inclined strata have generally been formed in their present positions. But this is certainly in perfect opposition to all the plainest dictates of reason: If such parts ever were covered with water, the whole Earth must have been covered to the same height; and where are such quantities of water to be produced? Or, if ever they existed, what is become of them? And, even if we could account for this necessary quantity of water, how is it possible that the matter of which these strata are formed could be supported, in a fluid state, in their inclined positions, during the process of their crystallization? In short, when we see men advancing such opinions as these, we are almost tempted to doubt whether there is any thing in the human mind, on which we can rely, to enable us to distinguish truth from error.

All those marine productions that are found in the more elevated strata must have been deposited in them previous to the elevation of these to their present position. There is not the smallest reason for supposing that these strata have ever been covered with water since that period; which is a thing, in every point of view, that seems physically impossible. The last great operations of water on the surface of the Earth are to be discovered only in the last formed, or alluvial strata; and these evidently show that it had only "bathed the feet of the highest mountains, covered the less elevated hills, and rendered the plains impassable."

So long as philosophers allow their minds to be in the smallest degree biassed by the account which Moses

gives of the creation of the world and the deluge, it is in vain to expect any thing like rational inquiry. What could be Cuvier's reason for endeavouring to make people believe that his theory accorded with the Mosaic account of the deluge, when it is evident that no two things can be more at variance? He supposes the surface of the Earth to have undergone many revolutions; and that at each the whole race of animals was almost entirely destroyed. Moses tells us, that the Earth was once covered with water, and a seed of all the different animals carefully preserved in an ark. Frenchmen! candour obliges us to own, that we are much indebted to you for many of the materials made use of in this inquiry. If the religious institutions of your country have hitherto prevented you from completing your discoveries, it is hoped that those times are now passed away; and that the day is fast approaching when, in every land, the truth shall be enabled to rear its head, and when science shall no longer be dragged at the heels of a false and delusive religion.

Buffon entertained the idea, that all mountains were more steep or precipitous towards the south and west than towards the north and east. But his views seem to have been more directed to the general decrease in height in the ranges of mountains, and the declivity of continents, than the form of individual mountains. His inferences are partly drawn from the greater rapidity of the current of rivers running towards the south and west, than of those running towards the north and east. He mentions, in particular, that long range of mountains, the Cordilleras, which descend so rapidly towards the west, whilst towards the east they are gradually lowered into vast plains, watered by the largest rivers in the world. When we attempt to apply the above rule of Buffon's to the form of individual

mountains, we do not find it by any means of such universal application as that which we have laid down for hills in the plains, in the northern latitudes, viz. that they are more steep on the west than on the east. It would require more observations to prove that his rule is even of general application.

In the form of mountains we find a very great difference, which has certainly arisen from the different state of the matter of which they were formed at the time of their elevation. This appears the more probable, as the same character generally extends to whole ranges. We find some with an easy slope on all sides; or at least they are easily accessible by man on all sides; others, again, have one or more sides almost as perpendicular as a stone wall. The matter of which these last are formed must, at the time of their elevation, have possessed a degree of hardness nearly equal to solid rock; otherwise they never could have retained their perpendicular form. It is generally amongst these that lakes are found. In the former kind, which have a more gentle slope, the matter must have been in a state of greater softness, so as to fill up all the hollows till the water found a declivity from their surface to the lower grounds. But in neither of these species of mountains, when situated in the higher districts, do we perceive any particular regularity in their form, in regard to any particular point of the compass.

We should naturally imagine that those mountains with their perpendicular rocky sides had been formed, or at least had attained their final elevation, posterior to the others, when the general strata had attained a greater degree of induration. Whether this had been the case, can be determined only by more particular observations on their relation to the horizontal strata,

and to the other species of mountains that may be found in their immediate vicinity.

The great original cause of the unevennesses on the Earth's surface had certainly been the unequal induration of the different strata during the process of the general condensation of the Earth itself. Had the whole matter of the Earth been of an uniform degree of firmness during the process of its condensation, all the parts would have yielded alike, and the whole surface would have been perfectly smooth. But if, whilst the internal nucleus continued in a state of elastic fluidity, the external strata had acquired a considerable degree of hardness, these strata would yield less readily to the condensing power than the internal parts, which would be continually decreasing in bulk. The outer strata would soon be more than sufficient for covering the internal nucleus; they must therefore have been broken and displaced in a variety of ways. But it does not appear that any general regularity had taken place in the breaking up of the strata; what regularities do appear seem to be found only in the order of their original formations, or are confined to those parts that have been acted upon by the currents of the sea.

Many of the horizontal strata have an undulated appearance. This we may ascribe to their having been bent by compression in the direction of their length or breadth, as well as condensed in the direction of their thickness, before they were completely hardened.

There is a law that seems of very general application to the sea coasts, both of the continents and islands of the world, viz. on all the western coasts the declivity of the land is more precipitous, and the water deeper, indicating, what we have already supposed, the former

existence of a current of water from west to east, washing away all the softer materials from the western coasts, and leaving, only such as were more capable of resisting the action of the water, but depositing on the eastern coasts shelving banks of sand, gravel, and other alluvial matter. But this law, though general, it is evident should not be looked for as universal; for larger or smaller islands lying off these western coasts must frequently have intercepted the force of this current, and altered its direction. We have very strong evidence from these coasts, besides their mere form, of the former existence of this current of the ocean: When we compare their strata with those of the islands that may happen to lie in their vicinity, we often discover such a correspondence as cannot but lead to the belief that they have formerly been united; and that part of the strata by which they had been connected has been washed away by the violence of the sea. But we do not see how such vast and complete devastation could be effected by any powers at present in existence, and acting upon strata of the present hardness of those rocks that are left behind. The quantity of matter removed in many cases has been immense: The strata, from what is left, must often have existed to a height considerably above the present level of the sea, and they have been removed to a depth far below it; and the matter has been completely swept away, leaving scarce a wreck behind. All rocks are more or less intersected with seams. The water makes its impressions first upon these, as being the most vulnerable parts; it widens these seams by degrees; thus dividing the rocks into blocks of various sizes; these, by the force of the waters, are removed from their beds, leaving the parts behind them to be acted upon in the same manner in their turn. It is true, that these blocks will afterwards

continue to be acted upon and wasted by the waters; but this is an operation that proceeds very slowly. That process by which the rocks are loosened and brought down in the form of blocks must always proceed much faster than that by which these blocks are so worn down as to be carried away by the waters; so that it is impossible to conceive such immense quantities of rock washed away without leaving a greater quantity behind in the state of loose blocks. In short, it seems impossible to account for the phenomena in question, without supposing them to have happened at a time before the strata were completely indurated; and when the waters of the ocean were moving with a strong current, and acting with a prodigiously greater force than any thing of the kind to be found at the present day. Sand-stone might then be reduced to loose sand, and the calcarious and other rocks to a still more attenuated state.

It will here naturally be asked, Can any cause be assigned for these former currents? Or, have we any reason to suppose that the waters of the sea were ever operated upon by any other powers than those by which they are at present influenced? We must answer, Yes. We see very evident reasons for a former current of the sea from west to east. Had the original nucleus of the Earth, with its atmosphere and all the matter suspended in it, been of one uniform or homogeneous density, a mere condensation of all these parts could have produced no relative motion amongst them. The parts would have retained the same comparative distances from one another, and the same relative positions; only their distances lessened. But there can be no doubt that the original atmosphere of the Earth would decrease in density upwards the same as at present, or at least this would be the case long before the

Earth came into its present position; it would revolve, too, in nearly the same time as the Earth itself round its own axis; and, consequently, the higher parts of that atmosphere must have had a much greater velocity than the surface of the solid nucleus, even at the equator. When the atmospheric matter became condensed, its rotatory velocity would still be increased during its precipitation, and the water formed of it must have had a motion from west to east with regard to the solid nucleus of the earth. Although we cannot say with any thing like precision what this relative motion of the water must have been, yet it is evident from the immense height of the original atmosphere, from whence these waters were formed, that it must have been very considerable.

Humboldt takes notice of sand-stone strata of an immense thickness, upwards of 9000 feet, on the western coast of South America—a thickness perhaps not to be met with in any other part of the globe. May we not be allowed to suppose that these strata have been produced by the original motion of the sea from west to east? If ever such a motion did exist, it is evident that it must have acted with peculiar force in the Pacific ocean, the most extensive body of water on the globe; it would carry along with it the matter as it was precipitated and granulated in the form of sand, until its progress was arrested by the rising continent of America, where it would be deposited, and afterwards consolidated in the form of sand-stone strata. This, however, would only lead us to infer that the principal effect of this primary motion of the waters of the sea from west to east must have been in the deposition of the matter of which the secondary strata are formed; and in this way it must have affected the form of continents.

But there have most probably been currents of the sea arising from another cause, which have continued to a later period; and which have committed those devastations we have been noticing on the strata after their partial consolidation.

We have already shown, that it is extremely probable that during the return of the Earth towards the Sun, the increase of the growing power of gravitation was not uniform or continuous: but that, owing to the eliptical figure of the Earth's orbit, this increase of power proceeded with periodical retrogradations—that it retrograded once in every revolution of the Earth round the Sun, viz. when it was moving from its perihelion to its aphelion; and that the figure of the Earth, whilst it was in a fluid state, must have had a constant tendency to adapt itself to these variations in the power of gravity: it would have a constant disposition towards greater oblateness when moving outwards to the aphelion, and the contrary when returning to the perihelion. As the solid matter of the Earth, however, became more and more indurated, it would less readily conform itself to these variations in the power of gravitation; but the waters, still retaining their perfect fluidity, would continue to obey them as before; when the Earth was moving towards the aphelion, the waters would move towards the equator; and when it was returning towards the perihelion, they would return towards the poles.

When the waters were spread out upon the wide circle of the equator, from the extent of high grounds in those parts, which would prevent the waters from breaking over them, they must soon have acquired the velocity of those parts themselves. When these waters were again returned towards the poles, they would at first have a tendency to move with that velocity which

they had acquired at the equator; this would give them a great relative velocity from west to east. The lands near the poles being in general less elevated, and the waters concentrated upon a smaller circle, the sea would break over all the lower grounds; and this motion might continue for a very long time, but with a velocity continually diminishing, till, in the outward motion of the Earth from the Sun, the waters again began to move towards the equator. Here, on the other hand, from their diminished velocity, the waters would have a relative motion from east to west, and the effects of this appear in the West India islands, the adjoining coast of America, and the eastern coast of the old continent.

There are few things that leave more evident marks behind them than currents of water, where they take place upon soft or loose materials that must yield to their force. Wherever they have prevailed, the rocks will be found washed bare on the side that had been opposed to the current; whilst on the opposite or sheltered side, there is almost always a sloping bank of gravel, sand, or other alluvial matter. If a general system of observations were instituted with this view, over all the alluvial formations of the Earth, the extent and direction of these latter currents might be ascertained with a considerable degree of precision.

The circumstance that the mountains near the equater are in general higher than those towards the poles, is another corroborative evidence of our theory. When the Earth was suffering a change in its figure from that of a more oblate spheroid by the increasing power of gravitation, so long as the elasticity of its matter was nearly perfect, this change of figure would follow almost immediately the change in that power; and the surface would continue nearly smooth. But as the

consolidation and induration of the strata increased, the contortions would be greater, and the surface less smooth. This would be the case more especially at the equator, where the secondary strata were thickest, and the change of figure, or the change in the latitudinal curve, would be the greatest. And hence the greater height of mountains there than towards the poles.

From the circumstances that must have attended this change of the figure of the Earth, we may account for another phenomenon that has often been taken notice of; viz. that in making experiments to ascertain the figure of the Earth, it is found that at small distances from the equator the plummet inclines more to the equator than it should do if the Earth were an ellipsoid of revolution of homogeneous density; thus denoting a greater density at the equator than at the poles. Now, it is evident, that whilst the change of figure was going on, the compressing force must have been greatest under the equator, forcing out the matter towards the poles; and therefore the matter must have been rendered at the time more dense at the equator. Had the fluidity and elasticity of the matter continued, a reaction would have taken place, and an equilibrium of density been restored; but as an induration was taking place all this time, the spring or elasticity of the matter could never recover itself; of course, the matter must have continued more dense there; and hence the inclination of the plummet that way.

There is another phenomenon which has much engaged the attention of geologists, and afforded matter of keen controversy, which we think may be satisfactorily accounted for by our theory of the change of the figure of the Earth. Along all the northern sea coasts there seem very evident remains of a former and higher

water-mark. This by some has been deemed a sufficient proof that the general level of the ocean has formerly been much higher than at present; and, consequently, that the quantity of water on the Earth has been so much diminished; whilst the difficulty of accounting for this diminution seems to have led others to doubt the reality of this high water-mark. But our theory accounts for the phenomenon by the elevation of the land at the poles, and not by the diminution of the quantity of water. When the Earth was assuming its present figure, there would be a constant rising of the land at the poles, and a depression of it at the equator. As the figure approached that oblateness which is due to the centrifugal force of the diurnal rotatory motion and the present power of gravity, this depression and elevation would proceed by very slow degrees; and it may be questioned if they have yet attained a maximum, or if the figure of the Earth has arrived at a state of perfect quiescence.

When a change of figure takes place in a solid indurated body, like the Earth, the change proceeds, not by a constant gradual yielding of the parts, but by starts and intermissions, with a rending of the parts, like a stone wall when it begins to fail. If this has been the case with the Earth, may not the rents thus produced, by forming a communication betwixt the central heat and the surface of the Earth, be the cause of earthquakes?

Conjoined with the phenomena of the Earth, we have those of the Moon, in corroboration of our theory. Our theory assigns to the Moon but a very light atmosphere, and a proportionate small quantity of water. Now, the mountains in the Moon are much higher, and the vallies much deeper, than those of the Earth. It has all that rugged appearance which the Earth would

have if there was in it almost no water, nor horizontal strata deposited by water, to fill up those hollows formed by the breaking up of the original nucleus and the indurated primary strata, when it was receiving its present figure.

CONCLUSION.

We have thus endeavoured to show the agreement of our general theory with the greater part of the principal phenomena of the planetary system. There are still, indeed, many phenomena which we have not attempted to account for by it. We have not attempted to account for the origin of comets, though forming a part of the same system; nor for the inclination of the axes of the planets to their orbits. But we see nothing in these at all opposed to our theory; and though we have not been able to account for them, it is hoped that future inquirers may.

It will perhaps be objected to our theory, that it is too mechanical; and that it does not give us that sublime idea of the Deity which the Mosaic account of the creation (absurdly called a theory) affords us, when it represents the universe as having been called into existence by mere volition:—"He said, Let there be light, and there was light." But however this may please the phantastical imaginations of some men, it is certainly altogether unlike any thing that we observe in nature. It is not for man to say what is suitable for the Deity to do. It is the business of the philosopher to discover what really is—not to determine what ought to be.

Although our theory is purely mechanical, there is none farther from excluding the idea of a Governing

Mind from the universe. It certainly never was objected to man's being endowed with a spirit—that the actions of his body are performed by the mechanical use of its members. Our theory supposes the planetary system to have gone through a regular mechanical process at its formation. But order always implies design; and so far from assigning to matter any original active properties, as the cause of that process, we have shown that it has derived these properties themselves from the order of nature at the creation. And in attributing all the active properties of matter to the motion of the system, this is in perfect agreement with what we observe in all the minor systems. In the animal and the vegetable kingdoms, no sooner does motion cease, than vital heat and cohesion fail, and the bodies immediately tend to dissolution; so in the great system of nature, if universal motion were to be stopped, gravity, heat, and every other property of matter would cease; and the whole would be reduced to a perfect chaos.

PART II.

MORAL PHILOSOPHY.

" Thus, have we found a true astrology;
Thus, have we found a new and noble sense,
In which alone stars govern human fates."

Night Thoughts.

CHAPTER I.

THE ANALOGICAL APPLICATION OF THE MECHANICAL THEORY TO THE SCIENCE OF MIND.

MECHANICAL Philosophy, as it applies to the motions of the planetary system, and the powers by which it is supported, has long been admired for its sublime simplicity, and regarded as that model of perfection which we would wish to see attained in the other sciences. This seems now happily within our reach, with regard to Moral Philosophy, by means of Analogy.

Astrologers formerly pretended to foretell the future destinies of men from the aspects of the heavenly bodies. We shall render the study of these bodies of more real importance to mankind, if we can really trace, by analogy, from the origin of the motions of these bodies, and the properties of the matter of which they are composed, the origin of the various propensities of human nature, and of those principles by which the

frame of society is upheld. We shall hence derive the elements of a system of moral philosophy, of the purest and most unalterable nature, for the regulation of individual conduct and the government of the world.

Nature in all its various departments seems to be governed by the same laws. Whatever properties we discover in matter, we find others exactly corresponding to them in mind. In matter we have inertia, gravitation or attraction, and repulsion; corresponding to these, we have steadiness, sympathetic affection, and aversion, in mind. In the material world, the power of attraction decreases as the square of the distance; so in mind, although we cannot measure the distance as we do in matter, we find that the impressions made on it depend greatly on the distance of the operating cause: a small object, when near, making more impression than a greater object at a distance. When an elastic body is acted upon by another, that action, instead of joining the two bodies together, frequently increases their distance by the recoil; so we find that men of opposite dispositions, by coming in contact, feel an aversion for each other that they never knew before. In matter, we find some bodies very soft and ductile; whilst others are of quite an opposite description: So there are some minds extremely pliable, and on which impressions are easily made; whilst others are of a most hard and obdurate nature. It is found, too, that mind as well as matter can be softened by external applications. In the collision of bodies, power is always lost; whilst by the mere combination of matter power is generated: So we find that by contention a man always loses influence in the world; but by winning the affections of others his power is uniformly increased.

In matter there are other properties besides mere inertia, attraction, and repulsion—or, perhaps, other

modifications of these properties—necessary to produce and support the organization of the animal and vegetable kingdoms; the former of which, and particularly Man, the head of it, seems to be the great object the Deity has had in view in the formation of this world. The three simple properties of inertia, attraction, and repulsion, seem only to have formed the solid basis on which the superstructure was to be built. Corresponding to this, in mind, other properties are necessary besides mere steadiness, affection, and aversion. These, indeed, constitute the great basis on which the fabric of the moral system rests; but without other properties, or other modifications of these properties, we should have had nothing but brute passion for the direction of human conduct. Without imagination, reason, and judgment, we should have none of those institutions on which all intellectual pleasure and happiness depend. Such are a few of the common properties of mind, compared with those of matter;—there can be little doubt that the analogy will hold good throughout the whole of the two systems, if accurately compared.

Let us now compare the origin of the planetary system, and the properties of matter by which it is upheld, with the origin of society and the principles of human nature, by which the frame of society is supported; and we shall again find the most remarkable agreement.

Our theory has shown* that, in the great bodies of the planetary system, the tendency of the individual particles, abstractedly considered, is to remain at rest, whilst their collective tendency is to move forward with a great velocity,—that these contrary tendencies,

* Part I. Origin of the Planetary System.

as individual particles, and as constituent parts of the great bodies of the system, is the cause of gravitation; and that the disposition of the individual particles to remain at rest is the cause of inertia, which gives stability to matter at rest, and power to it when in motion. So in the moral system, the first disposition of individuals is opposed to that of society: It inclines them to appropriate every thing to their own views and purposes. But no sooner do they look around them, than they perceive that they are but members of a great family; and that this disposition to appropriate every thing to themselves is inconsistent, not only with the good of society in general, but with their own interest as individuals: for if every one were acting upon that principle, without control, there could be nothing but rapine and confusion in the world. The consciousness of this makes them draw together for mutual protection, and to form those very barriers, the laws of society, which they themselves, as individuals, are nevertheless continually endeavouring to break through. It is these opposite dispositions of men, as individuals, and as members of society, that produce all the social attachments; and the more strongly the selfish spirit evinces itself, the more firmly do the individuals, as members of society, cling together for mutual protection. So that it is this very principle of selfishness, so odious in itself, that gives strength to those attachments, and stability and power to society.

In the material system, the interruption of motion is the cause of repulsion: so in morals, we every day observe that if one individual is interrupted in his pursuits by another, his mind evidently manifests an irritation and repulsion; or if any of the great movements

of society are interrupted, a convulsion frequently takes place which threatens to break up the whole social body.

In the solar system, all the orbital motions of the planets are regulated by their gravitation towards the sun. It is to this gravitation that the system owes its permanency, and the regularity of its motions: without it, the planets would all separate and wander into the regions of unknown space, and lose all the benignant and necessary influence of that great central body. So in every well regulated society, all its great pursuits are regulated by a steady regard to the Deity, or his laws —the eternal laws of justice and truth. Every deviation from these produces a corresponding disorder in society;—if we could suppose an entire abandonment of them, society would inevitably go to pieces.

It is from the sun that all the light and heat which enlightens and animates the whole vital creation are derived. So it is the light of the countenance of the Deity, shining through the medium of His works, that enlightens the human mind. It is the influence of His Spirit, acting through the same medium, that enlivens all our affections, and animates us in the discharge of our duty.

It will perhaps be objected to this, that a great part of mankind feel little or no regard for the works or the laws of the Deity; and that self-gratification absorbs almost their whole attention and care. Moralists, indeed, generally allege that pleasure and pain are the two great objects of desire and aversion amongst mankind. But this idea proceeds from their not considering man in his two distinct characters, as an individual, and as a member of society. Regarding man abstractedly, as an individual, this observation of moralists is just; but consider him, again, as a member of society,

his selfish views are always modified, and frequently overruled, by a regard for the laws of the Deity, as they direct him in his duty towards the society in which he lives. So sensible are mankind that attention to the general good of the world is their great duty, that even the sensualist dares not confess that self-gratification is his object: He knows that society will not accept of this from one of its members; he would therefore have the world to believe that he intends the improvement of society, by adding to its enjoyments. So that here the parallel betwixt the material and moral systems still holds complete.

As in the material world a body feels, more or less, every impression that is made on every other body connected with it, so in society we feel or sympathize with every other member with whom we are associated. That particular affection which certain individuals feel for each other is to be compared to elective attraction in physics.

It is by the gravitation of the particles of a planet towards its centre that its parts are held together, and the regularity of its figure preserved. Corresponding to this, we have the regard of mankind for the social system: On this are founded, a regard for the laws, and all those virtues that tend to the order, the stability, and welfare of society.

The regard of man for the Deity corresponds to the gravitation of the particles of a planet towards the sun. On this are founded, all the great and respectable virtues, which tend to the government of the conduct and the passions. This constitutes what is called Principle, or the Religious Principle, and which forms so essential a part in the moral character of man. By it a man proceeds in the regular discharge of all his duties, with an undeviating adherence to justice and

truth, uninfluenced by circumstances, or the opinion of the world, as the parts of a planet continue to revolve with perfect regularity round the sun, notwithstanding the storms that may prevail upon its surface. Do we wish to impress man with the religious principle ? Let us make him sensible of the power, the wisdom, the justice and benificence of the Deity, as manifested in the works of creation. How impious, then, must those systems of religion be that are founded on the supposed imperfections and disorders of the system of nature !

Having found such a striking and general correspondence betwixt the two systems of Matter and Mind, we come now to inquire, Whether the two can be separated ; or if they form necessary constituent parts of the same system of nature ; or,

WHAT IS MIND ?

MIND has generally been supposed active, and matter passive. Mind has been supposed to be something altogether distinct from matter ; and to it all the motions of matter have been ascribed. If this were the case, it is not the body, but the soul of man, that ought to suffer fatigue from bodily exercise. This, however, is so far from being the fact, that the mind is frequently at ease when the body is suffering the greatest fatigue. It is close study and reflection that fatigue the mind ; and this commonly takes place when the body is at perfect rest. We have then recourse to some moderate exercise, as a relief and refreshment to the mind. The mind exercises no absolute power over the body : all the motions of the body are performed by the mechanical use of its members ; and even this mechanical use of its members cannot

be effected without the assistance of other external matter. We cannot walk without the earth to enable us to make a progressive motion; nor can we continue this motion without the external air to inflate the lungs. All the impressions which mind receives are through the medium of matter. But for the impressions of the external world, we could have no consciousness but that of mere existence ; nor even of that, but for the sensations of the matter of which the body is composed. Mind has no more absolute power than matter has : whether it acts or is acted upon, it is only through the medium of matter. When we consider of mind, in relation to the system of nature, we say it is that which gives consciousness—which contrives, resolves, &c. But if we endeavour to form an abstract idea of mind, without regard to any part of the material system of nature, we find the thing impossible. We can form no idea whatever of mind, without matter for the manifestation of its properties.

Neither should we be surprised at this: the same thing occurs with regard to our ideas of matter. Is it required of us, What is matter ? We say, it is that which constitutes the substance of all those solid, or gravitating, or hard, or fluid, or elastic, or moving bodies, which we observe in the system: but we do not conceive these properties of solidity, or gravity, or hardness, or fluidity, or elasticity, or motion, to be essential to matter. Accordingly, we always ask, What are the causes of these properties ? And, on investigation, we find that they are derived from the order of the system. But, deprive the system of this order, and, consequently, matter of these properties, and we can form no idea of it whatever. In this imaginary state, we give it the name of chaos ; a thing to which we may

attach a negative, but no positive idea: if ever we attempt to form any positive idea of it, it flies like a phantom from the mind.

Thus, all the properties of matter are derived from the order of the system, and, consequently, from mind; and all the ideas of the mind are derived originally from matter. Matter and mind are therefore inseparable. Why does a tree not walk and speak as a man

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Moreover, we find that all the mutual affections of bodies for one another are owing to their being related in the same system. But matter and mind mutually affect each other; they must therefore constitute the same system; otherwise, they could not enter into all those mutual relations, and feel all those mutual affections. Matter and mind, therefore, form necessary constituent parts of the same system. Mind, however, is not a property of matter, but a principle: for it possesses itself properties similar to all those which matter does; and it would be absurd to call that a property which possesses all other properties. Matter and mind must have had one common existence; they have been organized and refined by the same process, and continue to be regulated by the same laws.

Matter and mind must both have existed from eternity: for we cannot conceive the one to have existed without the other; neither can we conceive how that which at any time was not, could call itself into existence. It has not, however, been always in the same state: for we see evident marks in the planetary system, and in this earth in particular, of its having gone through a regular process of formation. It may have gone through many changes, and it may undergo many more; although we may be unable to discover the means by which such future changes may be effected.

WHAT IS THE SOUL OF MAN?

"What then am I?"

Is there any thing essentially different in the nature of man from the rest of the material creation? Or, Does he only form a part in the general gradation of refinement in the system of nature?

We find matter in its rudest state endowed with the powers of inertia, gravitation, and repulsion. We have traced these to the motion of the system, and a tendency to equilibrium. But we cannot conceive matter without mind to have any tendency or disposition either to motion or to equilibrium. The mutual gravitation, then, of two of the rudest bodies of matter affords no less evidence of their being pervaded with mind, than the most refined sympathies of human nature do of the existence of the soul of man. Hence, there is no such thing in nature as matter absolutely inanimate.

In a more refined state, matter possesses the power of crystallization, and forming itself into regular and beautiful figures. It even manifests, to a certain extent, the faculty of choosing and rejecting, in the property of elective attraction.

In a higher state, we find it formed into vegetables, with a power of absorbing and assimilating other matter into their own natures, and propagating their kinds. We see them even sending out their roots in quest of nourishment, and their flowers and leaves following the light.

In a still higher state, it is formed into brute animals; which, in addition to the above, have the power of locomotion. They even exhibit the more evident manifestations of what we call mind: they not only choose and reject, but they show the strongest affec-

tions and aversions for particular objects. They frequently even exhibit a considerable degree of reason—a faculty which is generally supposed to be altogether peculiar to man. By reason, we mean the faculty of observing and comparing, and making use of their observations in their after conduct. There are many brute animals which, if they had the faculty of speech, and the use of hands, would make a superior figure in the world to some of the human species.

At the head of the creation stands man, the chief servant of the Deity on earth, with a form of body and constitution of mind well adapted to give him the command over the rest of the world. The form of his body is not only suitable for the greatest variety of action, but likewise for making observations, and for entering into the greatest variety of relations with all the other parts of the natural system. The constitution of his mind corresponds with the organization of his body; and enables him to apply all his bodily powers with the greatest effect. But still, man just acts, and is acted upon, like the other parts of the natural system. He is impressed with the same powers of inertia, gravitation, and repulsion, as what we call inanimate matter. He possesses the same powers of absorption and assimilation, and propagating his kind, that vegetables do. He has the powers of locomotion, of observing and choosing, with the lower animals. But the great distinguishing features in the character of man, are those faculties with which he is endowed of tracing the relation of cause and effect, and communicating his ideas to others of his kind. By the former, he is enabled to render all the elements of nature subservient to his purposes; by the latter, he enters into associations for the accomplishment of objects beyond the power of separate individuals.

Thus, man stands merely at the head of a regular gradation in the system: there is nothing essentially different in his nature from the rest of the creation. His superiority arises entirely from superiority of organization.

Whatever differences of opinion there may be amongst the learned with regard to the sciences of phrenology, physiognomy, and pathognomy, the common conduct of men affords a tacit acknowledgment of their be ef that the constitution, the sentiments, and resolution of the mind, correspond with the organization, the expressions, and movements of the body, The most unobservant of men, aye, even brute animals, will be forming opinions of the dispositions and intentions of men, from their outward appearance, their countenance, and gestures.

But though the mind is dependent for its constitution on the organisation of the body, we do not say that the organization of the body is independent of the mind: they are certainly born and do grow up together, and mutually affect each other. If the body, even after it is brought to maturity, can be distorted by the passions, there can be little reason to doubt, that, during the period of its growth, the form of its parts may be affected by the dispositions of the mind.

And again, although the differences observable in the dispositions and faculties of different men, are owing to differences in their bodily organization, there is no reason to suppose that the differences in the organisation of the brain, the seat of the thinking organs of the mind, of different men, are at all in proportion to the differences to be found in the courses they pursue, or to the results of their intellectual labours. The smallest difference of the inclination of surface in the middle of a continent will send its waters to opposite

quarters of the globe; so a very little difference of original disposition often determines men to the most opposite courses through life: and with regard to the actions they perform, these depend much upon the circumstances they may be placed in, or the incidents they meet with in the world. As in physics, matter possessing the most active properties may lie dormant from not being placed in circumstances to bring these powers into action; so men of the most powerful minds may pass through the world unobserved for want of some sufficient motive to excite them to action, and to give manifestation of their powers: for "great objects make great minds."

From analogy, we should deem it probable that the differences of intellectual powers amongst men are not greater than those of the body. We see men of the greatest bodily strength passing their time in a listless inactivity, and doing almost nothing; whilst others much inferior in strength, but inured to habits of industry, are performing prodigious quantities of labour. So it is with regard to intellectual productions: "The habit of application," says Culbertson, "generally does more than the greatest strength of intellect ever attempts." If this analogy holds good, (and we see no reason to doubt it,) it affords great encouragement to all who feel disposed to exercise their intellectual faculties, not to be too much discouraged by false apprehensions of natural incapacity; it should at the same time be a warning to them not to look for success without much industrious application.

The same principle of analogy, by which we have discovered the general correspondence betwixt matter and mind, leads us likewise to the personification of the human soul. The soul of man has its periods of growth, of maturation, and of decay; it is supplied with food

and nourishment from external events, and, like the body, it suffers fatigue from labour, and requires periodical rest and repose, to recruit its exhausted powers. It possesses the power of moving from object to object, and from one idea to another; it builds and it pulls down; it cultivates and reaps. It possesses the means of acquiring knowledge, and powers of action corresponding to those of the body. The soul pervades the whole body of man; and the centre of its affections, like the centre of gravity in a physical body, rests in the heart, the centre of the body. The mind has a taste, a relish or disrelish, for certain things. It has a faculty of hearing, or of receiving information from report. It may likewise be said to have the sense of smell, as certain agreeable or disagreeable sensations are conveyed to it from distant objects, with which it is not immediately connected. But of all the organs by which the body acquires knowledge, the eye infinitely surpasses all the others, both in the extent and correctness of the information which it affords. Corresponding to the bodily eye, we have Reason, the no less valuable faculty of the mind.

It is a remarkable fact, that notwithstanding all that has been said and written on the subject, mankind have hitherto been in utter ignorance as to the true nature of reason. In a scientific point of view, reason is commonly understood to be a wonderful and mysterious faculty, by which we are enabled to penetrate the intricate and profound recesses of nature; but which cannot be applied to such things as appear evident at first sight. "But though reason," says Mr Smith,* "is undoubtedly the source of the general rules of morality, and of all moral judgments which we form by

* Theory of Moral Sentiments.

means of them; it is altogether absurd and unintelligible to suppose that the first perceptions of right and wrong should be derived from reason, even in those particular cases upon the experience of which the general rules are formed. These first perceptions, as well as all other experiments upon which any general rules are founded, cannot be the object of reason, but of immediate sense and feeling." Reasoning in this way, Smith infers that the only use of reason, in forming rules of morality, is by forming general rules, by means of induction, from observations made in a vast variety of instances. But if this were the case, then Cain was not culpable in murdering his brother Abel, having had no previous instances from whence to infer that murder was a crime. Yet nothing would appear more absurd, surely, to a man of common sense, than such an assertion.

But the truth is, that reason is neither more nor less than the plain simple *Eye of the Mind;* and is to be used in every respect as we use the bodily eye. As the first link of a chain is as much an object of perception to the bodily eye as the last, or as the whole chain itself, so reason, acting as immediately as the eye, the first perceptions of mental objects are not less suited for the cognizance of reason than a long train of circumstances. "A vast variety of instances" is not necessary to teach the mind what is right or what is wrong: the eye of the mind has a faculty of perceiving what is comely, and what is deformed—what is straight, and what is crooked; and a moral injury cannot be committed, but it sees that it is a deviation from the path of moral rectitude, and is contrary to those laws of natural affection by which mankind are linked together. But though it is not necessary, as Smith alleges it is, to make observations in a vast variety of instances, in

order to form the general rules of morality; yet it is frequently necessary to take into consideration a vast variety of circumstances, before we can apply these rules to a particular case. Thus, although murder is one of the greatest and most evident of all moral crimes, yet the taking the life of another is often justifiable, as in self-defence; or, it is frequently to be recommended, as in cases of public justice.

The views taken by the learned are not always more correct than those of the vulgar. This seems particularly the case with regard to reason; and may perhaps be accounted for by the former being more actuated by an anxiety for distinction and display than the simple elucidation of the truth. In common life, the man who takes, or who can be brought to take, a fair, candid, and unprejudiced view of things or circumstances as they really are, without bias or partiality, is termed a man of reason, or a reasonable man; and the appellation is certainly just. But amongst the learned, reason is understood to be a powerful faculty, by which a man not only makes great discoveries in nature, but by means of which he can either establish or confute any opinion or proposition that may be advanced, whether false or true, as he may be inclined or called upon to do. In order to effect this, he must frequently form groundless hypotheses, false analogies, and apply false colourings; he must bring distant objects into such a line with the eye as to make them appear parts of one and the same object; by partial coverings, he must represent the parts of other objects as separated and detached; and if he can effect his purpose by these and other means of deception, he is then reckoned a most acute reasoner. But this, in fact, is not reasoning: it is an abuse of reason, and a darkening of the mind. The effusions of the logician

are always to be suspected; and the more so the greater art he exhibits.

From the above view which philosophers have taken of reason, they have formed the famous science of *Logic*, or the *Art of Reasoning*. Now, we may just as well talk of the *Art of Seeing* as the *Art of Reasoning*. If we wish to enable a man to reason on any subject, we must make him acquainted with that subject. If we wish to enable him to reason on any art or science, we must make him acquainted with the principles of that art or science; and this is to be done only by laying the subject plainly before him. Is it asked then, Are there no means by which reason can be assisted? Yes, there certainly are. The assistance which the telescope and the microscope afford to the bodily eye, that do arithmetic and geometry afford to the mind, in bringing near that which is too remote, and analizing that which is too minute, for the unaided powers of reason. Yet there are some in whom the eye of the mind is sealed by disease, or hood-winked by prejudice and erroneous education, who cannot be made to see very clearly even by these means. But logic is calculated only for the purposes of deception: By a certain arrangement of words, it forms false figures and representations, which mislead and deceive; but it never can assist us in investigating the truth. In the darker ages of Christianity, when priestcraft and superstition were in the meridian of their power, enveloping a great part of the world in the grossest mental darkness; and when the great desideratum was, What were the most powerful means by which certain articles of belief could be impressed upon the minds of men, or doubts and objections suppressed and confounded? Then logic was fostered and encouraged as the most useful of all the sciences.

The intelligent part of mankind have now been long pretty well aware of the evils which the world has sustained from this misapplication of the reasoning faculty. But it has happened here, as in many other cases where the equipoise is once destroyed, the correction of one abuse has led to an opposite extreme. Logic was employed by the priesthood to make men believe all things. It has now fallen into the hands of others of an opposite character, who would make men disbelieve all things. These latter affect a wisdom above their fellows. They affect a wisdom above what is written by the hand of nature. They would argue down all the natural feelings, and all the intimations of the senses, as so many foolish prejudices. In short, they would reason away every thing that generally goes by the name of common sense. When the human mind, however, shall have recovered its equilibrium, this abuse of reason will certainly be discarded, as not less unworthy of an intelligent being than the former impostures and deceptions of the priesthood. Indeed, logic has already been gradually losing ground ever since the dawn of reason which began at the Reformation of religion; and as the light increases it must cease to be numbered amongst the cultivated sciences.

There is as an art in the application of the mental as well as the bodily powers; there is an art in analizing mental as well as material subjects. There is an art in composing as well as an art in building; but the science of composition is no more the art of thinking, than the science of architecture is the art of seeing. To acquire knowledge in the application of the intellectual, as of the bodily powers, much observation is necessary both of the works of nature and the works of art; and both are to be perfected by practice.

People are often surprised at the acute observations

of children; but the fact is, that as the eye of the body is more acute in youth than in old age, so is reason, the eye of the mind. But though the eye of the mind is more acute in youth, the mind itself is not so strong, neither is its knowledge so great or extensive, for want of experience or practice: its decisions are therefore less to be trusted than in maturer years.

Small as this discovery with regard to the nature of reason may appear, it is hoped that it is one that will be of infinite advantage to the world, as it will encourage mankind to make a more free, unincumbered, and extensive use of that faculty than has ever hitherto been thought of. The world in general has been led to suppose, that there is something in the use of reason by ordinary minds, that implies a certain degree of arrogance and presumption; and that it is only the more intelligent and enlightened that should at all pretend to use it. But will even the most common of mankind any longer submit to be told, that they must not use their eyes? That they may not use the eye of their mind as well as their bodily eye? A man may just as well submit to have his bodily eyes tied up, and be led blindfolded through the world, as to have the use of his reason denied him.

The free use of reason has always been the dread of of impostors and tyrants. And well it may—for by it every delusion shall be detected and exposed. By it, the vail of the temple of superstition shall be rent in twain. By it, those who at present sit in darkness shall see a great light. By it, the rod of the oppressor shall be broken: liberty shall be given to the captive, and freedom to them that are bound. But upon the same principle by which reason overturns whatever is false or unjust, it establishes and confirms whatever is just and true. It is a terror to evil-doers, but a praise

and protection to them that do well. It is true, that that thing which has hitherto gone by the name of reason has often been most dangerous to the peace of society: it has proved a most powerful instrument, in the hands of the factious and the designing, for misleading the multitude. But true reason is the reverse of this; as it consists in calm observation and reflection, nothing can be so efficacious in keeping all the passions completely under control, and in fortifying the minds of the people against the designs of the turbulent demagogue.

CHAPTER II.

APPLICATION OF THE MECHANICAL THEORY TO THE QUESTION OF THE FREE-AGENCY OF MAN.

"Others, apart, sat on a hill retired
In thoughts more elevate, and reason'd high
Of Providence, foreknowledge, will, and fate;
Fix'd fate, free will, foreknowledge absolute;
And found no end, in wand'ring mazes lost."—MILTON.

THE vanity of man is not content with being placed merely at the head of the works of the Deity in this world: he persuades himself that his mental part has had an origin different from that of the other parts of the creation, and superior to those laws which prevail over them. He persuades himself that "there is in mind a certain independent self-governing power which there is not in body; and in consequence of which, there is a great difference between the relation of motive and action, and that of cause and effect in physics; and by means of which a person, in all common cases, may, at his own discretion, act either according to, or in opposition to, any motive, or combination of motives, applied to him; while body, in all cases, irresistably undergoes the change corresponding to the cause, or combination of causes, applied to it."

If man really possessed an independent self-governing power, this would render very incomplete the analogy betwixt matter and mind. But the idea of the

free-agency of man is a mere deception. We flatter ourselves that we are acting according to the dictates of our own free will, or that we are merely following our own inclination, when, in fact, we are urged on by passions or desires, which the smallest reflection may satisfy us are the results of external affections; and that we are then not more independent of the common laws of nature than a stone when it is falling to the earth, by the power of gravitation, which might with just as much reason be said to be following its own natural inclination. We may think that we are affording the most incontestible proof of our independence, when we refuse to yield to certain attempts that are made to move us; but when, in reality, our refusal to yield arises from our attachment to some other object; as in physics, a ball may resist an external impulse from its attachment or cohesion to some other body.

There is nothing more consonant with the feelings and opinions of mankind, than that certain causes, or motives, produce certain effects on the human mind. It is on the confidence in this that are founded all those laws, customs, and institutions of the world that are intended to influence and regulate the minds of men. It is true that it is much more difficult to trace the relation of cause and effect in the phenomena of mind than in those of matter. So it is more difficult to trace that relation in the impulsion or resistance of fluids than in the case of solids; but no man having any pretension to scientific knowledge would from hence assert, that the relation of cause and effect is not as certain and invariable in the one case as in the other. And as man stands in a greater variety of relations to the other parts of the system of nature than any other created being, it ought not to surprise us that it is often

extremely difficult, and frequently beyond our power, to determine the effect of motives upon a being placed in the midst of such a variety of motives, affections, and attachments, as those by which he is generally influenced. We very often err in calculating the effects which certain motives should have upon certain men; but when we discover the cause of our mistake, we do not find that it has arisen from any irregularity in those laws which regulate the dispositions of the human mind, but from our own ignorance of the particular views, affections, or attachments of the men; or from some error in our mode of the application of the motives.

Much of the misunderstanding and diversity of opinion that have taken place among the learned with regard to the free-agency of man, may be traced to that endless source of error, the doctrine of absolute power, by which a cause is understood to be an absolute power producing an absolute impression on whatever object it is applied to, instead of a relative action producing a relative effect. As in physics, the same power does not always produce the same effect, even in the simple case of impulsion, and where the body acted upon has nothing to resist its motion but its own inertia, (whatever different opinions have been entertained on this subject,) the effect depends as much on the quantity of matter acted upon as the power employed; or, the power being the same, the effect is not less influenced by the body acted upon being attached to, or connected with, other bodies; so with regard to mind, it would be absurd to expect that the same motive should in every case produce the same absolute effect. It is well known that the same motive will, as it necessarily must, produce very different effects upon different minds, or upon the same mind at different

times. Before we can calculate with any degree of certainty upon the effect which any motive will have upon a certain mind, we must acquaint ourselves with all its affections and attachments at the time it is to be operated upon.

That motives have a certain effect upon the human mind is a feeling almost universal amongst mankind. That it is not more explicitly avowed seems owing to that vanity by which men are led to imagine themselves superior to motives, and an apprehension entertained by a certain class, that subjecting men to the necessary influence of motives would do away human responsibility; and that of course it would be most prejudicial to the cause of religion and morality. This has caused the subject to be argued with a degree of keenness, and even rancour, that is very unfavourable to the discovery of truth; but it is hoped that a more liberal and candid examination of the subject, will show that the apprehensions of these men are as groundless as the arguments, by which they endeavour to controvert the necessary influence of motives, are ill founded.

It has been asserted, that, if it is true that equal motives produce equal effects upon mind, if we were to place a hungry ass betwixt two bundles of hay equally inviting, the beast must stand still and starve to death, being unable to turn to either, because there are equal motives to both. But is it not very evident here that though the ass is placed betwixt two very equal things, two equal bundles of hay, it is at the same time placed betwixt two that are very unequal, a bellyful of hay on the one hand, and starvation on the other? The ass would very soon cease to compare the bundles of hay: It would compare a bellyful of hay against starvation, in which comparison there is a very weighty motive that would soon decide the question. To this, it

will perhaps be answered, that the case supposes the ass to be comparing the two bundles of hay; and that though the choice of the two bundles forms but a small part of its consideration, compared with its inclination to eat, yet it still forms some part, which, according to the doctrine of necessity, must have some motive to determine it. To this it may be observed, that the mind considers only one thing at any one instant of time; and in making a comparison betwixt two things, it turns first to the one and then to the other. Whilst the mind of the ass is vibrating betwixt the two, its inclination to eat makes it fall to the one it is considering at the instant.

It has also been said, that the mind often acts without a motive; as when a man lifts a guinea from a heap. In acquiring a guinea there is a considerable motive;—in choosing a particular guinea from a heap, where they are all alike, there is none at all. If one lies more convenient for lifting than the rest, he will take that; if not, he will take the one his mind happens to be considering at the instant he resolves to lift one.

Father Buffier, in his Treatise of First Truths, proposes the following experiment for the decision of the Necessarian doctrine:—"If," says he, "I am not free, it must necessarily be decreed, that, within a quarter of an hour hence, I either shall, or shall not, raise my hand thrice successively; so that I cannot alter this necessary determination. This being supposed, in case I lay a wager on one side rather than on the other, I can be a winner only on one side; that is, either by laying that I shall raise my hand thrice, or that I shall not. If you seriously pretend that I am not free, you cannot reasonably refuse the following offer:—I will lay you a thousand guineas to one, that, with respect to moving my hand, I shall do quite the reverse of what

you may contend for, and you shall take which side you please; so that, if you lay that I shall raise my hand, I lay that I will not; and if you lay that I shall not, I lay that I will. If you think this offer advantageous, you must accept it, and if you do not think it advantageous, whence can such an idea arise, but from the necessary and invincible opinion you have of my being free, and that it is in my power to make you lose such a wager, not only once, but a million of times, if you should have the folly to repeat it so often." Did the sagacious father not discover that he would here have the very powerful motive of gaining a thousand guineas and saving one, to take the opposite side of the one contended for?

Amongst all those who have attempted to confute what is called the Necessarian doctrine, none seems to have given greater satisfaction to those who have espoused the same side of the question than Dr Gregory; he giving what is considered mathematical demonstration of its absurdity. It is easy to satisfy those who are predisposed to believe; and it is only such that his demonstrations will convince.

"The chief instances* in which Dr Gregory demonstrates the evident diversity between the relation of cause and effect in physics, and that of motive and action in human conduct, are the following: It is a well-known law in physics, and is demonstrated by Sir Isaac Newton, in the first corollary to his three laws of motion, that if two impelling forces or causes, differing in direction, be at the same time applied to a body at rest, such that by the separate action of one of them, the body would have moved over a certain right line in a given time; then shall the body, by the joint ac-

* Scott's Inquiry, p. 259.

tion of both forces, move exactly in the diagonal of a parallelogram, of which these two lines form the adjacent sides; and it shall complete the diagonal in the same time that it would have completed either of the sides by the corresponding separate force.

"To apply this law to the case of motive and action: If a porter be offered a guinea for every mile that he may carry a letter in a certain direction, it is probable that he will perform the task. If he be offered, at another time, a like bribe for carrying a letter in another direction, making an angle with the former, it is likewise most probable that the task will be accomplished. But, now, let us suppose, that at the same instant of time he is offered both sums, to perform at once both tasks; and let us inquire, what will be the result? According to the doctrine of the Necessarians, that there is a constant and proportional conjunction between motive and action, the porter cannot go in either of the two directions, but must travel in the precise diagonal between them. 'It is folly,' says Dr Gregory, 'for him to make a pretence of thinking, and ridiculous for him to make any words about it; for go he *must* in that precise direction, as sure as ever a projectile moved in a curve; and pretty nearly for the same reasons.'" (*Philos. and Liter. Essays*, § 9.)

"If the porter go in one of the proposed directions, without at all regarding the other, one of the proposed motives is completely separated from its corresponding action; and if he go in neither of the proposed directions, but remain at rest where he originally was, both the motives are completely separated from their corresponding actions. As, therefore, we are well aware that no such journey in the diagonal will ever take place; 'and as,' says Dr Gregory, 'it may even be doubted whether the most confident assertors of Mr

Hume's doctrine will risk a single guinea on such an experiment;' here is a fair *deductio ad falsum*, from the principles of the Necessarians that mind is inert."

Mathematics constitute a very useful science; but to use them without previously and maturely considering all the circumstances of the subject to which they are to be applied, only evinces the folly of him who makes the application. In the above instance, the two cases of the physical body and the porter are stated in such a way that they cannot be compared. The physical body is supposed to be impelled, not towards two given points, but in two given directions, making an angle with each other. To place the porter in a parallel situation, he should be hired to carry the letters, not as letters are usually carried, to two given points, but in two given directions, making likewise an angle with each other. Let him be offered, for instance, five guineas to carry a letter five miles to the westward, and other five to carry another letter five miles to the southward, in the same time; he will then, if he understands his business, certainly carry both the letters a little more than seven miles to the southwest; for he will find that by so doing he shall implement what is required of him, and gain ten guineas, instead of five which he would only have got for carrying one of the letters either due west or due south. Thus, he will move precisely in the diagonal of the parallelogram, like the physical body impelled by two forces, provided he is influenced by no other motive or impediment; and it is evident that the physical body might likewise be disturbed or interrupted by obstacles in its course, as well as the porter.

As another, what is called mathematical demonstration of the diversity between the relation of cause and effect in physics, and that of motive and action in hu-

man conduct, and of the absurdity of the Necessarian doctrine, it is observed,* that, "It is well known in physics, that if a body be impelled in a certain direction by a certain force, and at the same time it be impelled in the direction exactly opposite by another less force; it will proceed in the first direction, with an impulse equal to the difference between these forces. Thus, if the body be impelled eastward with a force as 10, and westward with a force as 4, it will move eastward with a force as 6. But this analogy by no means holds in the case of motive and action. 'If motives of equal strength oppose one another,' says Dr Gregory, 'it is held that no action can take place, as they mutually counteract each other; but it is thought, that if motives of unequal strength directly oppose one another, the stronger will not only prevail, but have its full effect, as if it were not opposed at all.

"'Thus, a porter assured of a guinea a mile for going due east, and of as much for going due west, as fast as he could, if his face chanced to be due north or south, it is conceived, must remain at rest till some new motive occur to determine his choice and direct his course. But it is conceived, that if he were assured of a guinea a mile for going east, and only of half-a-guinea a mile for going west, he would go east at the rate required of him, and earn the guineas, notwithstanding the constant conjunction of motive and action; just as he would have done if no such opposite motive as the offer of the half-guineas had been applied.' Of this doctrine, our author observes, that, "in plain English it amounts to this, that when ten are deducted from ten, there can remain nothing; but that when four, or five, or six, are deducted

* Scott's Inquiry. p. 261.

from ten, there will remain ten; which is absurd."—(*Essays*, § 10.)

Here, again, we have a most unfair representation of the case; we are called upon to consider the motion of the porter, or the rate at which he walks, as indicating the kind and degree of motive force applied to him. Whereas that force does not fall on the body, but on the mind; and is manifested by the determination it gives to the mind, and not by the motion of the body. And, again, that determination can be estimated only by the quantity of counteracting force that would be required to overcome or resist it.

If a porter is offered ten guineas to go a certain distance eastward, and four guineas to go the same distance westward, in a certain time, he will certainly go eastward in that time, just as if the four guineas had not been offered. Yet the four guineas are not without their effect: although the porter will move at the same rate as if they had not been offered, he will not move with the same intensity or determination; or, in other words, he will be more easily stopped. Without withdrawing the four guineas, let him be offered the smallest sum more than other six, and he will then certainly move to the westward;—a proof that the four guineas have had their full proportional effect.

The same reasoning applies to the guineas and half-guineas. Although the porter will go to the east for a guinea a mile, notwithstanding the offer of half-a-guinea a mile to go west, just as fast as if these half-guineas had not been offered; yet he will not go with the same determination; as the smallest thing more than other as many half-guineas will turn him, and make him go to the west.

If the porter is offered equal sums to go east and west at the same time, this will put him for some time in a

state of suspense: but, like the ass with the two equal bundles of hay, he finds that by standing still he loses both sums; he therefore resolves to proceed; and goes in that direction towards which his mind happens to be turned at the instant that his resolution to proceed is fixed.

It has been alleged by some that the term, *necessary agent*, implies a contradiction. But this is mere idle cavilling; and in confutation of it, we need only instance a water-mill. Water is certainly the agent by which it is turned; and water is certainly a necessary agent: it necessarily acts by the power of gravitation.

Amongst other objections to the necessary influence of motives, it has been observed that man does not always yield to the strongest motive, or follow the greatest good. It is true that man frequently follows that which even his own conviction tells him is not his true interest. But this argues nothing against the necessary influence of motives. The man's mind is under the influence of some previous and stronger contrary impression; and although the latter motive may be the strongest of those applied to it at the time, it cannot instantly counteract the impetus it has received from those previous impressions. A motive of even very small power, but operating for a long period of time, may give to the mind a bias which scarcely any subsequent experience, even aided by reason and conviction, can altogether eradicate or overcome: and hence the danger of contracting early evil habits.

The same reasoning applies to the opinion we form of others. When early and strong impressions are made on our minds in favour of any one, the inertia of our minds, or their disposition to continue in the state in which they are put, does not allow these impressions to be instantly destroyed by every subsequent miscon-

duct of that person: hence the proverbial partiality and blindness of parents and friends. When this property of inertia prevails in the mind to an excess, it goes by the name of prejudice, and often makes a man appear very ridiculous. But although the excess of inertia in the mind makes a man appear foolish, yet a due proportion of it forms, not only a salutary, but a necessary part, in the constitution of the moral system. A deficiency of inertia produces a volatility and unsteadiness of character. If it were utterly wanting—if every fault or failing in our friend were completely to obliterate all former good impressions with respect to him, there could be no permanency in our attachments, nor durability in friendship: it would perfectly destroy all confidence amongst mankind in one another.

It can hardly be necessary to call the attention to the perfect analogy in all this betwixt mind and matter. Before any certain disposition can be impressed on the one, or motion communicated to the other, all previous contrary dispositions in the one, and motions in the other, must first be subdued; to effect which, a power is required at least equal to that to be overcome. It must be evident to every one who pays the smallest attention to the subject, that there is an analogy betwixt motive and action in human affairs, and cause and effect in physics, that is not merely casual or accidental. It becomes the duty, then, of every candid enquirer, instead of using vain cavilling, dispassionately to examine how far this analogy extends; and, upon due examination, it will certainly be found to be universal. All the objections that have been urged against this analogy only amount to this, That it is more difficult to trace the relation of motive and action in human conduct, than of cause and effect in physics; but this difficulty does not make the actual

relation of motive and action one whit less certain, although we may be thereby misled in our calculations respecting them. This should make men cautious and charitable in forming their opinion of the actions of others.

We may here observe, that this difficulty in tracing the relation of motive and action, is a wise ordination of the Deity, as it is one of the greatest checks in the way of the ambitious, to prevent them from making the great bulk of mankind mere machines, subservient to their purposes and designs.

There are certain classes of men, as we have already observed, who see, or affect to see, great danger to religion and morality in the doctrine of the necessary-agency of man. But when we trace their apprehensions to their true source, we find that these men are not so much afraid of the subversion of religion and morality as of certain institutions, in the permanency of which they feel themselves deeply interested. They are more jealous with regard to the honour of man than the honour of the Deity. The free-agency of man is quite inconsistent with the foreknowledge of the Deity. It is impossible to conceive on what principle the foreknowledge of even the Deity can extend to actions or events which may be brought about or prevented by the mere humour or caprice of man. This is so evident, that it is admitted even by some of the advocates for man's free-agency. But rather than give up the freedom of the human will, they relinquish the foreknowledge of God; they deny that the prescience of the Deity extends to the voluntary or contingent actions of men. Do these men consider that they hereby exclude the Deity from the foreknowledge of almost every thing that is of importance to mankind? Or, what sort

of religion is it, that consists with such doctrine as this?

The doctrine of the absolute free-agency of man excludes the Deity from the government of the world. Man is certainly the prime agent employed by the Deity in the government of the world; but if man acts independently and without control, this supposes the Deity to have consigned the absolute government of the world to him; than which, nothing can be more impious and absurd.

The free-agency of man likewise separates man from the great Governor of the world. If we disallow the influence of external motives, we separate man from the rest of the system of God; we preclude all correspondence betwixt God and man, but through the worse than doubtful medium of priests and miracles. And hence we see a very strong reason for the part that the priesthood have uniformly taken in this argument.

In order to avoid the imputation of excluding the Deity from the government of the world, the advocates for the free-agency of man tell us, that "the Supreme Being, without exerting any positive control over the actions of man, may, by the exertion of infinite wisdom and infinite power, cause the creature unknowingly to promote the great purposes of its creator." Now, how is it possible to conceive infinite wisdom and infinite power to be exerted, without exerting a positive control? The control is not the less positive that it does not immediately produce the ultimate object. If a steam-engine is employed to raise water, and that water afterwards turned upon a mill for the purpose of grinding corn, the agency of the steam is not less positive, nor less conducive to the ultimate object than that of the water, though not so immediately applied.

Neither is the agency or control of motives the less positive that they cause us *unknowingly* to promote the purposes of the Creator. The Deity has been compared to a prudent parent possessing the entire command of a set of children, who can make them, in certain cases, promote his own views, without imposing any restraint upon their wills. But this is tacitly admitting all that we assert, that, in this case, motives are employed, though unobserved by the children, and that the motives do produce their natural effect. The assertors of the free-agency of man seem obstinately to shut their eyes against the obvious fact, that the human will is merely the determination given to the mind by external motives.

With regard to the effect of the doctrine of the necessary-agency of man on morality, it ought to be sufficient that we prove it to be the law of nature, and consequently the law of God. This ought especially to satisfy those who profess to be religious, and to place their whole trust in God. But, as not only the religious, but mankind in general, seem in this, as in other cases, more guided by the ultimate object than by the laws which the Governor of the world has given us for the direction of our conduct in the attaining of it, it may be necessary to show that the adoption of those laws can lead to no evil.

Are we afraid that the doctrine of necessity will make men careless and indifferent as to their moral conduct, by affording them a plea of non-responsibility? Such fears are altogether groundless. It has always been allowed that men are not accountable for the intellectual powers with which they are endowed; yet we find them in general more jealous of the opinion of the world with regard to these than even their moral principles; insomuch that it has long been observed

that in common a man would rather be reckoned a knave than a fool; and hence men are certainly more assiduous in the cultivation of their intellectual faculties than of their moral dispositions. Why, then, should we be afraid of placing their moral dispositions on the same foundation? But the very terms in which the doctrine of necessity is couched exclude the possibility of man's relaxing in his endeavours towards moral perfection: the regard for the good will and respect of the world must necessarily continue, as they have hitherto done, to stimulate man to make himself useful and agreeable to all with whom he is connected.

Are we afraid that the doctrine of necessity will lead us to less just, or less favourable, opinions of the moral conduct of others? If necessity is the law of nature, the opinions founded upon it must be just; and there is no reason to apprehend, that upon the whole it will lead us to form a less favourable judgment of others. If the moral conduct of man depends upon his original constitution and the circumstances in which he happens to be placed, we cannot, indeed, ascribe to him either absolute merit, or absolute demerit. But this will not alter our estimation of the man: We do not ascribe any merit to gold for being a purer metal than silver; nor do we blame copper for being less pure than either silver or gold; yet we put very different values upon these three metals. So the good and the virtuous will continue to command our esteem and respect; while the base and the vicious will excite our contempt and disapprobation; though we may not conceive that the one should merit the reward of everlasting pleasures, nor the other deserve everlasting torments. If we consider man as a free independent moral agent, having an absolute control over all his actions, we are naturally led to view every moral tres-

pass as an act of pure wilful devilishness, and deserving of the highest punishment. But the doctrine of relative necessity represents things to us in a very different light. It shows us that the errors or trespasses of a man are owing either to an original unhappiness of constitution, or to his being unfortunately situated in regard to external circumstances. And thus, whilst we must disapprove of, or detest, the actions themselves, we have still a feeling of pity for the man; more especially when, which is frequently the case, his evil propensities are conjoined with better qualities.

The necessary influence of motives explains to us, in the most natural way, the manner in which man is seduced from the path of moral rectitude; and thereby shows us where to apply the cure. By mental applications, we can soften the hardened and strengthen the weak. By means of motives, vice can be discouraged and virtue stimulated. Bad affections can be neutralized, by combining with their objects the evil consequences with which they are necessarily attended. By the same means, we can even render that object repulsive which formerly attracted. The necessary influence of motives affords, too, the only sure grounds of confidence in the efficiency of the means we employ. We may, indeed, err in their application, from the imperfection of our knowledge; (and wherein is it that we may not err?) but the motives themselves can never fail in producing their legitimate effect. So far, then, from the necessary influence of motives being an erroneous or a dangerous doctrine, it is it alone that can afford us any rational explication of the variety and change observable in the actions of men; and it is to it alone that we can look with any sort of confidence for the improvement of morality.

How different is the doctrine of the free-agency of

man. According to it, the origin of evil is accounted for by the introduction upon earth of a spirit essentially evil—a devil—a strange monster, surely, to be allowed to exist in the kingdom of God. It precludes all possibility of improvement by means of motives. It is in vain, according to it, that we represent virtue in all its beauty, or vice in its native deformity: the result depends entirely upon the humour or caprice of the man to whom they are held forth; and the probability is, that "fallen, degenerate man" will choose the evil, and refuse the good. But there are still two things, and it is supposed there are only two, that can safely be trusted to man's choice, viz. reward and punishment; and the advocates for man's free-agency trust to these alone in the government of the world. Nothing tends more to the degradation of man than a system of government founded on rewards and punishments. It generates mean servility and slavish fear. It is calculated only for slaves; and has a tendency to reduce to that state all who are put under its influence.

The doctrine of the free-agency of man admits of no other means for his reformation and improvement than the free grace of God. But if we ask why God does not extend this free grace to all, we are told, "He will have mercy on whom he will have mercy, and whom He will He hardeneth." Precious doctrine this! but it is the necessary result of the doctrine of man's free-agency, which has been defended with an anxiety and keenness, as if the very existence of religion and morality depended upon it;—a doctrine itself fraught with the grossest absurdities, and necessarily leading to perfect blasphemy, by representing the Deity as arbitrary and capricious, and the author of absolute evil.

But though it is not to rewards and punishments that we should look for leading or stimulating men to the

discharge of their duty; yet these form a very necessary part in the moral government of God. When the mind of man is impressed with a disposition to move in a given direction, it can no more stop or change that disposition of itself, than a physical body can stop or alter the direction of its own motion. In both cases, a resisting or changing force is necessary. It has long been observed that success begets confidence; and it certainly increases power. When the mind succeeds in the object of its desire, that object continues to carry the mind along with an accelerating velocity: it hurries it rapidly past other objects, whose attractive powers have not time to produce any sensible effect upon it. When this disposition of the mind happens to be evil, it would necessarily produce an endless accumulation of mischief, if there were not some resisting or counteracting powers to arrest its progress. But, happily, their are such powers in nature. Every individual naturally resists or resents an injury done to him, as society does those committed upon its members. The offender is thereby either made to retrace his steps, if he is of an yielding disposition, or, if of a hardened and obstinate nature, and the collision violent, and the reacting body at the same time of sufficient firmness to resist it, it may end in his destruction. How foolish or wicked, then, is it to preach up passive obedience, or non-resistance. Reaction, or resistance, is an universal, an useful, and necessary law of nature, for arresting the progress of moral and physical evil.

The good and beneficent dispositions likewise require to be encouraged and assisted, to enable them to persevere against the difficulties and resistance they often experience. And there certainly is, too, a disposition, both in men and in the general order of nature, to reward virtue. There is, indeed, a very common com-

plaint in the world, of the perverseness and ingratitude of men: but when we bestow a candid consideration on these complaints, we generally find that they proceed from misplaced sympathy, or from neglected and disappointed vanity and officiousness. Neglect for favours received frequently proceeds from an extreme of a spirit of independence; a spirit of the greatest utility when kept under due control, as it makes man put a higher value upon what he can do for himself than the assistance he may receive from others; and, consequently, is a most powerful stimulus to individual exertion.

Whatever system of religion or morality we adopt, the actual existence of moral evil must be admitted; but it is only upon the principle of relative necessity that we can account for it without any imputation against the laws or the government of God. The existence of moral evil is perfectly consistent with the existence of relative, though not of absolute perfection. Man fondly imagines to himself the possibility, and even the former actual existence, of a state of absolute perfection, and freedom from all evil. But absolute perfection in physics or in morals involves a physical or moral impossibility. All the laws of God are relatively perfect; they are perfect in answering the purposes for which they were designed; with whatever evils or inconveniences they may occasionally be attended. When we wish to lift or raise a heavy body, we reckon its weight a great inconvenience; or when a stone falls and crushes a man to death, we think its gravitation a serious evil. But without gravitation the material system could not be held together. When our view is interrupted by an opaque body, we wish it were transparent. But if all matter were transparent, then every thing would be seen through, but nothing would be seen. We often

consider the want of durability in matter an imperfection; but if bodies were perfectly hard, unyielding, and indestructible, they never could be altered, changed, or improved into such forms as we may wish or require; and whatever degree of original perfection we might suppose in those bodies or substances that are destined for the use of man, they would soon cease to afford pleasure or delight if unsusceptible of variation or change.

Vices generally begin from a want of proper attention, or an imperfection in our reason; or they begin in the indulgence of our appetites and affections in circumstances of perfect innocence; but being carried along with us into other situations and circumstances, they become highly criminal. But to complain that our reason does not prove to us a perfect guide, or that the eye of the mind does not see clearly through a long succession of events, especially where other events, not immediately connected with those we are examining, intervene, is as absurd as to complain that the bodily eye cannot see through opaque matter. Neither is it possible that men can instantaneously change their appetites and affections upon every change of circumstances; even though these appetites or affections should be proved to them to be wrong. Every change in morals, as in physics, requires not only a counteracting power, but likewise a certain time for that power to operate. This is a necessary consequence of the principle of inertia, or steadiness, which causes men to persevere, for a time, in their evil as well as their good propensities; and without which principle there could be no stability, and consequently no confidence among men. When this principle prevails in a high degree in good dispositions, it is termed firmness;

but with regard to the evil propensities, it is termed obstinacy; and becomes a very great imperfection in the human character.

The social principle, or desire to please, leads us to adopt the existing manners, and, consequently, the vices, of others. But without this principle, men could never associate together; and hence we should be destitute of all the pleasures and advantages of the social state.

We reckon death the greatest of all evils; but without the decomposition of former living bodies, animal and vegetable, to afford nourishment for the present, these could never exist. Without death, we could not live. Had none of the former generations of men died, there could neither have been means for the subsistence of the present, nor room for them on the earth;—the present race of men could never have had being.

This world affords a subject of endless improvement; but without the smallest prospect of ever attaining to any thing like absolute perfection. Endless improvement and absolute perfection are very different things. Absolute perfection excludes all possibility of farther improvement; and in approaching to it, every discovery made would lessen the number of discoveries remaining to be made. But in a state of endless improvement, such as this world presents to us, every discovery only widens the field of inquiry, and the field of improvement; and hence gives more scope for the exercise of the highest powers of the human mind. A state of absolute perfection is a mere chimera;—a state that never did, and never can exist, according to the present laws of nature, or according to any order of nature that we can conceive. The existence of evil seems

even necessary to our perception or consciousness of good; without evil to be corrected, resisted, or overcome, we could have no idea of goodness. Without darkness, we could have no idea of light. The idea of absolute perfection, in short, reduces the whole system of nature, at least in as far as respects our senses, to a perfect nonentity.

THE END.

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